SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, JANUARY 1, 1904.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

*SCIENTIFIC INVESTIGATION AND PROGRESS.**

AT the weekly services of many of our churches it is customary to begin with the reading of a verse or two from the Scriptures for the purpose, I suppose, of putting the congregations in the proper state of mind for the exercises which are to follow. It seems to me we may profit by this example, and accordingly I ask your attention to Article I. of the Constitution of the American Association for the Advancement of Science, which reads thus: 'The objects of the association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.'

The first object mentioned, you will observe, is 'to promote intercourse between those who are cultivating science in different parts of America'; the second is 'to give a stronger and more general impulse and more systematic direction to scientific research'; and the third is 'to procure for the labors of scientific men increased facilities and a wider usefulness.' Those who are familiar with the history of the association are well aware that it has served its purposes admirably, and I am inclined to think that those who have been

* Address of the retiring president of the American Association for the Advancement of Science, St. Louis meeting, December 28, 1903.

in the habit of attending the meetings will agree that the object which appeals to them most strongly is the promotion of intercourse between those who are cultivating science. Given this intercourse and the other objects will be reached as a necessary consequence, for the intercourse stimulates thought, and thought leads to work, and work leads to wider usefulness.

While in 1848, when the association was organized and the constitution was adopted, there was a fair number of good scientific investigators in this country, it is certain that in the half century that has passed since then the number of investigators has increased very largely, and naturally the amount of scientific work done at present is very much greater than it was at that time. So great has been the increase in scientific activity during recent years that we are apt to think that by comparison scientific research is a new acquisition. In fact there appears to be an impression abroad that in the world at large scientific research is a relatively new thing, for which we of this generation and our immediate predecessors are largely responsible. Only a superficial knowledge of the history of science is necessary, however, to show that the sciences have been developed slowly, and that their beginnings are to be looked for in the very earliest times. Everything seems to point to the conclusion that men have always been engaged in efforts to learn more and more in regard to the world in which they find themselves. Sometimes they have been guided by one motive and sometimes by another, but the one great underlying motive has been the desire to get a clearer and clearer understanding of the universe. But besides this there has been the desire to find means of increasing the comfort and happiness of the human race.

A reference to the history of chemistry will serve to show how these motives have operated side by side. One of the first great incentives for working with chemical things was the thought that it was possible to convert base metals like lead and copper into the so-called noble metals, silver and gold. Probably no idea has ever operated as strongly as this upon the minds of men to lead them to undertake chemical experiments. It held control of intellectual men for centuries and it was not until about a hundred years ago that it lost its hold. It is very doubtful if the purely scientific question whether one form of matter can be transformed into another would have had the power to control the activities of investigators for so long a time; and it is idle to speculate upon this subject. It should, however, be borne in mind that many of those who were engaged in this work were actuated by a desire to put money in their purses—a desire that is by no means to be condemned without reserve, and I mention it not for the purpose of condemning it, but to show that a motive that we sometimes think of as peculiarly modern is among the oldest known to man.

When the alchemists were at work upon their problems, another class of chemists were engaged upon problems of an entirely different nature. The fact that substances obtained from various natural sources and others made in the laboratory produce effects of various kinds when taken into the system led to the thought that these substances might be useful in the treatment of disease. Then, further, it was thought that disease itself is a chemical phenomenon. These thoughts, as is evident, furnish strong motives for the investigation of chemical substances, and the science of chemistry owes much to the work of those who were guided by these motives.

And so in each period as a new thought has served as the guide we find that men

have been actuated by different motives, and often one and the same worker has been under the influence of mixed motives. Only in a few cases does it appear that the highest motives alone operate. We must take men as we find them, and we may be thankful that on the whole there are so many who are impelled by one motive or another or by a mixture of motives to take up the work of investigating the world in which we live. Great progress is being made in consequence and almost daily we are called upon to wonder at some new and marvelous result of scientific investigation. It is quite impossible to make predictions of value in regard to what is likely to be revealed to us by continued work, but it is safe to believe that in our efforts to discover the secrets of the universe only a beginning has been made. No matter in what direction we may look we are aware of great unexplored territories, and even in those regions in which the greatest advances have been made it is evident that the knowledge gained is almost insignificant as compared with that which remains to be learned. But this line of thought may lead to a condition bordering on hopelessness and despondency, and surely we should avoid this condition, for there is much greater cause for rejoicing than for despair. Our successors will see more and see more clearly than we do, just as we see more and see more clearly than our predecessors. It is our duty to keep the work going without being too anxious to weigh the results on an absolute scale. It must be remembered that the absolute scale is not a very sensitive instrument, and that it requires the results of generations to affect it markedly.

On an occasion of this kind it seems fair to ask the question: What does the world gain by scientific investigation? This question has often been asked and often answered, but each answer differs in some respects from the others and each may be suggestive and worth giving. The question is a profound one, and no answer that can be given would be satisfactory. In general it may be said that the results of scientific investigation fall under three heads—the material, the intellectual and the ethical.

The material results are the most obvious and they naturally receive the most attention. The material wants of man are the first to receive consideration. They can not be neglected. He must have food and clothing, the means of combating disease, the means of transportation, the means of producing heat and a great variety of things that contribute to his bodily comfort and gratify his esthetic desires. It is not my purpose to attempt to deal with all of these and to show how science is helping to work out the problems suggested. I shall have to content myself by pointing out a few of the more important problems the solution of which depends upon the prosecution of scientific research.

First, the food problem. Whatever views one may hold in regard to that which has come to be called 'race suicide,' it is certain that the population of the world is increasing rapidly. The desirable places have been occupied. In some parts of the earth there is such a surplus of population that famines occur from time to time, and in other parts epidemics and floods relieve the embarrassment. We may fairly look forward to the time when the whole earth will be overpopulated unless the production of food becomes more scientific than it now is. Here is the field for the work of the agricultural chemist who is showing us how to increase the yield from a given area and, in case of poor and worn-out soils, how to preserve and increase their fertility. It appears that the methods of cultivating the soil are still comparatively crude, and more and more thorough inves-

tigation of the processes involved in the growth of plants is called for. Much has been learned since Liebig founded the science of agricultural chemistry. It was he who pointed out some of the ways by which it is possible to increase the fertility of a soil. Since the results of his investigations were given to the world the use of artificial fertilizers has become more and more general.

But it is one thing to know that artificial fertilizers are useful and it is quite another thing to get them. At first bone dust and guano were chiefly used. Then as these became dearer, phosphates and potassium salts from the mineral kingdom came into use.

At the Fifth International Congress for Applied Chemistry, held at Berlin, Germany, last June, Dr. Adolph Frank, of Charlottenburg, gave an extremely interesting address on the subject of the use of the nitrogen of the atmosphere for agriculture and the industries, which bears upon the problem that we are dealing with. Plants must have nitrogen. At present this is obtained from the great beds of saltpeter found on the west coast of South America—the so-called Chili saltpeter—and also from the ammonia obtained as a by-product in the distillation of coal, especially in the manufacture of coke. The use of Chili saltpeter for agricultural purposes began about 1860. In 1900 the quantity exported was 1,453,000 tons, and its value was about $60,000,000. In the same year the world's production of ammonium sulphate was about 500,000 tons, of a value of somewhat more than $20,000,000. Of these enormous quantities about three quarters finds application in agriculture. The use of these substances, especially of saltpeter, is increasing rapidly. At present it seems that the successful cultivation of the soil is dependent upon the use of nitrates, and the supply of nitrates is limited. Unless something is done we may look forward to the time when the earth, for lack of proper fertilizers, will not be able to produce as much as it now does, and meanwhile the demand for food is increasing. According to the most reliable estimations indeed the saltpeter beds will be exhausted in thirty or forty years. Is there a way out? Dr. Frank shows that there is. In the air there is nitrogen enough for all. The plants can make only a limited use of this directly. For the most part it must be in some form of chemical combination as, for example, a nitrate or ammonia. The conversion of atmospheric nitrogen into nitric acid would solve the problem, and this is now carried out. But Dr. Frank shows that there is another, perhaps more economical, way of getting the nitrogen into a form suitable for plant food. Calcium carbide can now be made without difficulty and is made in enormous quantities by the action of a powerful electric current upon a mixture of coal and lime. This substance has the power of absorbing nitrogen from the air, and the product thus formed appears to be capable of giving up its nitrogen to plants, or, in other words, to be a good fertilizer. It is true that this subject requires further investigation, but the results thus far obtained are full of promise. If the outcome should be what we have reason to hope, we may regard the approaching exhaustion of the saltpeter beds with equanimity. But, even without this to pin our faith to, we have the preparation of nitric acid from the nitrogen and oxygen of the air to fall back upon.

While speaking of the food problem, a few words in regard to the artificial preparation of foodstuffs. I am sorry to say that there is not much of promise to report upon in this connection. In spite of the brilliant achievements of chemists in the field of synthesis it remains true that thus

far they have not been able to make, except in very small quantities, substances that are useful as foods, and there is absolutely no prospect of this result being reached within a reasonable time. A few years ago Berthelot told us of a dream he had had. This has to do with the results that, according to Berthelot, are to be brought about by the advance of chemistry. The results of investigations already accomplished indicate that, in the future, methods will perhaps be devised for the artificial preparation of food from the water and carbonic acid so abundantly supplied by nature. Agriculture will then become unnecessary, and the landscape will not be disfigured by crops growing in geometrical figures. Water will be obtained from holes three or four miles deep in the earth, and this water will be above the boiling temperature, so that it can be used as a source of energy. It will be obtained in liquid form after it has undergone a process of natural distillation, which will free it from all impurities, including, of course, disease germs. The foods prepared by artificial methods will also be free from microbes, and there will consequently be less disease than at present. Further, the necessity for killing animals for food will no longer exist, and mankind will become gentler and more amenable to higher influences. There is, no doubt, much that is fascinating in this line of thought, but whether it is worth following, depends upon the fundamental assumption. Is it at all probable that chemists will ever be able to devise methods for the artificial preparation of foodstuffs? I can only say that to me it does not appear probable in the light of the results thus far obtained. I do not mean to question the probability of the ultimate synthesis of some of those substances that are of value as foods. This has already been accomplished on the small scale, but for the most part the synthetical processes employed have involved the use of substances which themselves are the products of natural processes. Thus, the fats can be made, but the substances from which they are made are generally obtained from nature and are not themselves synthetical products. Emil Fischer has, to be sure, made very small quantities of sugars of different kinds, but the task of building up a sugar from the raw material furnished by nature—that is to say, from carbonic acid and water—presents such difficulties that it may be said to be practically impossible.

When it comes to starch, and the proteids which are the other chief constituents of foodstuffs, the difficulties are still greater. There is not a suggestion of the possibility of making starch artificially, and the same is true of the proteids. In this connection it is, however, interesting to note that Emil Fischer, after his remarkable successes in the sugar group and the uric acid group, is now advancing upon the proteids. I have heard it said that at the beginning of his career he made out a program for his life work. This included the solution of three great problems. These are the determination of the constitution of uric acid, of the sugars and of the proteids. Two of these problems have been solved. May he be equally successful with the third! Even if he should be able to make a proteid, and show what it is, the problem of the artificial preparation of foodstuffs will not be solved. Indeed, it will hardly be affected.

Although science is not likely, within periods that we may venture to think of, to do away with the necessity of cultivating the soil, it is likely to teach us how to get more out of the soil than we now do, and thus put us in a position to provide for the generations that are to follow us. And this carries with it the thought that, unless scientific investigation is kept up,

these coming generations will be unprovided for.

Another way by which the food supply of the world can be increased is by relieving tracts of land that are now used for other purposes than the cultivation of foodstuffs. The most interesting example of this kind is that presented by the cultivation of indigo. There is a large demand for this substance, which is plainly founded upon esthetic desires of a somewhat rudimentary kind. Whatever the cause may be, the demand exists, and immense tracts of land have been and are still, devoted to the cultivation of the indigo plant. Within the past few years scientific investigation has shown that indigo can be made in the factory from substances, the production of which does not for the most part involve the cultivation of the soil. In 1900, according to the report of Dr. Brunck, Managing Director of the Badische Anilin- and Soda-Fabrik, the quantity of indigo produced annually in the factory 'would require the cultivation of an area of more than a quarter of a million acres of land (390 square miles) in the home of the indigo plant.' Dr. Brunck adds: "The first impression which this fact may be likely to produce, is that the manufacture of indigo will cause a terrible calamity to arise in that country; but, perhaps not. If one recalls to mind that India is periodically afflicted with famine, one ought not, without further consideration, to cast aside the hope that it might be good fortune for that country if the immense areas now devoted to a crop which is subject to many vicissitudes and to violent market changes were at last to be given over to the raising of breadstuffs and other food products." "For myself," says Dr. Brunck, "I do not assume to be an impartial adviser in this matter, but, nevertheless, I venture to express my conviction that the government of India will be rendering a very great service if it should support and aid the progress, which will in any case be irresistible, of this impending change in the cultivation of that country, and would support and direct its methodical and rational execution."

The connection between scientific investigation and health is so frequently the subject of discussion that I need not dwell upon it here. The discovery that many diseases are due primarily to the action of microscopic organisms that find their way into the body and produce the changes that reveal themselves in definite symptoms is a direct consequence of the study of the phenomenon of alcoholic fermentation by Pasteur. Everything that throws light upon the nature of the action of these microscopic organisms is of value in dealing with the great problem of combating disease. It has been established in a number of cases that they cause the formation of products that act as poisons and that the diseases are due to the action of these poisons. So also, as is well known, investigation has shown that antidotes to some of these poisons can be produced, and that by means of these antidotes the diseases can be controlled. But more important than this is the discovery of the way in which diseases are transmitted. With this knowledge it is possible to prevent the diseases. The great fact that the death rate is decreasing stands out prominently and proclaims to humanity the importance of scientific investigation. It is, however, to be noted in this connection that the decrease in the death rate compensates to some extent for the decrease in the birth rate, and that, if an increase in population is a thing to be desired, the investigations in the field of sanitary science are contributing to this result.

The development of the human race is dependent not alone upon a supply of food but upon a supply of energy in available

forms. Heat and mechanical energy are absolutely essential to man. The chief source of the energy that comes into play is fuel. We are primarily dependent upon the coal supply for the continuation of the activities of man. Without this, unless something is to take its place, man is doomed. Statistics in regard to the coal supply and the rate at which it is being used up have so frequently been presented by those who have special knowledge of this subject that I need not trouble you with them now. The only object in referring to it is to show that, unless by means of scientific investigation man is taught new methods of rendering the world's store of energy available for the production of heat and of motion, the age of the human race is measured by the extent of the supply of coal and other forms of fuel. By other forms of fuel I mean, of course, wood and oil. Plainly, as the demand for land for the production of foodstuffs increases, the amount available for the production of wood must decrease, so that wood need not be taken into account for the future. In regard to oil, our knowledge is not sufficient to enable us to make predictions of any value. If one of the theories now held in regard to the source of petroleum should prove to be correct, the world would find much consolation in it. According to this theory petroleum is not likely to be exhausted, for it is constantly being formed by the action of water upon carbides that in all probability exist in practically unlimited quantity in the interior of the earth. If this be true, then the problem of supplying energy may be reduced to one of transportation of oil. But given a supply of oil and, of course, the problem of transportation is solved.

What are the other practical sources of energy? The most important is the fall of water. This is being utilized more and more year by year since the methods of producing electric currents by means of the dynamo have been worked out. There is plainly much to be learned before the energy made available in the immediate neighborhood of the waterfall can be transported long distances economically, but advances are being made in this line, and already factories that have hitherto been dependent upon coal are making use of the energy derived from waterfalls. The more rapidly these advances take place the less will be the demand for coal, and if there were only enough waterfalls conveniently situated, there would be no difficulty in furnishing all the energy needed by man for heat or for motion.

It is a fortunate thing that, as the population of the earth increases, man's tastes become more complex. If only the simplest tastes prevailed, only the simplest occupations would be called for. But let us not lose time in idle speculations as to the way this primitive condition of things would affect man's progress. As a matter of fact his tastes are becoming more complex. Things that are not dreamed of in one generation become the necessities of the next generation. Many of these things are the direct results of scientific investigation. No end of examples will suggest themselves. Let me content myself by reference to one that has of late been the subject of much discussion. The development of the artificial dye-stuff industries is extremely instructive in many ways. The development has been the direct result of the scientific investigation of things that seemed to have little, if anything, to do with this world. Many thousands of workmen are now employed, and many millions of dollars are invested, in the manufacture of dye-stuffs that were unknown a few years ago. Here plainly the fundamental fact is the esthetic desire of man for colors. A colorless world would be unbearable to him. Nature accustoms him to color in a great variety of

combinations, and it becomes a necessity to him. And his desires increase as they are gratified. There seems to be no end to development in this line. At all events, the data at our disposal justify the conclusion that there will be a demand for every dye that combines the qualities of beauty and durability. Thousands of scientifically trained men are engaged in work in the effort to discover new dyes to meet the increasing demands. New industries are springing up and many find employment in them. As a rule the increased demand for labor caused by the establishment of these industries is not offset by the closing up of other industries. Certainly it is true that scientific investigation has created large demands for labor that could hardly find employment without these demands.

The welfare of a nation depends to a large extent upon the success of its industries. In his address as president of the British Association for the Advancement of Science given last summer Sir Norman Lockyer quotes Mr. Chamberlain thus: "I do not think it is necessary for me to say anything as to the urgency and necessity of scientific training. * * * It is not too much to say that the existence of this country, as the great commercial nation, depends upon it. * * * It depends very much upon what we are doing now, at the beginning of the twentieth century, whether at its end we shall continue to maintain our supremacy or even equality with our great commercial and manufacturing rivals." In another part of his address Sir Norman Lockyer says: "Further, I am told that the sum of £24,000,000 is less than half the amount by which Germany is yearly enriched by having improved upon our chemical industries, owing to our lack of scientific training. Many other industries have been attacked in the same way since, but taking this one instance alone, if we had spent this money fifty years ago, when the Prince Consort first called attention to our backwardness, the nation would now be much richer than it is, and would have much less to fear from competition."

But enough on the purely material side. Let us turn to the intellectual results of scientific investigation. This part of our subject might be summed up in a few words. It is so obvious that the intellectual condition of mankind is a direct result of scientific investigation that one hesitates to make the statement. The mind of man can not carry him much in advance of his knowledge of the facts. Intellectual gains can be made only by discoveries, and discoveries can be made only by investigation. One generation differs from another in the way it looks at the world. A generation that thinks the earth is the center of the universe differs intellectually from one that has learned the true position of the earth in the solar system, and the general relations of the solar system to other similar systems that make up the universe. A generation that sees in every species of animal and plant evidence of a special creative act differs from one that has recognized the general truth of the conception of evolution. And so in every department of knowledge the great generalizations that have been reached through the persistent efforts of scientific investigators are the intellectual gains that have resulted. These great generalizations measure the intellectual wealth of mankind. They are the foundations of all profitable thought. While the generalizations of science belong to the world, not all the world takes advantage of its opportunities. Nation differs from nation intellectually as individual differs from individual. It is not, however, the possession of knowledge that makes the efficient individual and the efficient nation. It is well known that an individual may be very learned and at the same time very inefficient. The question is, what use does he

make of his knowledge? When we speak of intellectual results of scientific investigation, we mean not only accumulated knowledge, but the way in which this knowledge is invested. A man who simply accumulates money and does not see to it that this money is carefully invested, is a miser, and no large results can come from his efforts. While, then, the intellectual state of a nation is measured partly by the extent to which it has taken possession of the generalizations that belong to the world, it is also measured by the extent to which the methods by which knowledge is accumulated have been brought into requisition and have become a part of the equipment of the people of that nation. The intellectual progress of a nation depends upon the adoption of scientific methods in dealing with intellectual problems. The scientific method is applicable to all kinds of intellectual problems. We need it in every department of activity. I have sometimes wondered what the result would be if the scientific method could be employed in all the manifold problems connected with the management of a government. Questions of tariff, of finance, of international relations would be dealt with much more satisfactorily than at present if the spirit of the scientific method were breathed into those who are called upon to deal with these questions. It is plain, I think, that the higher the intellectual state of a nation the better will it deal with all the problems that present themselves. As the intellectual state is a direct result of scientific investigation, it is clear that the nation that adopts the scientific method will in the end outrank both intellectually and industrially the nation that does not.

What are the ethical results of scientific investigation? No one can tell. There is one thought that in this connection I should like to impress upon you. The fundamental characteristic of the scientific method is honesty. In dealing with any question science asks no favors. The sole object is to learn the truth, and to be guided by the truth. Absolute accuracy, absolute fidelity, absolute honesty are the prime conditions of scientific progress. I believe that the constant use of the scientific method must in the end leave its impress upon him who uses it. The results will not be satisfactory in all cases, but the tendency will be in the right direction. A life spent in accordance with scientific teachings would be of a high order. It would practically conform to the teachings of the highest types of religion. The motives would be different, but so far as conduct is concerned the results would be practically identical. I need not enlarge upon this subject. Unfortunately, abstract truth and knowledge of facts and of the conclusions to be drawn from them do not at present furnish a sufficient basis for right living in the case of the great majority of mankind, and science can not now, and I do not believe it ever can, take the place of religion in some form. When the feeling that the two are antagonistic wears away, as it is wearing away, it will no doubt be seen that one supplements the other, in so far as they have to do with the conduct of man.

What are we doing in this country to encourage scientific investigation? Not until about a quarter of a century ago can it be said that it met with any encouragement. Since then there has been a great change. Up to that time research was sporadic. Soon after it became almost epidemic. The direct cause of the change was the establishing of courses in our universities for the training of investigators somewhat upon the lines followed in the German universities. In these courses the carrying out of an investigation plays an important part. This is, in fact, the culmination of the course. At first there were not many following these courses, but it was not long

before there was a demand for the products. Those who could present evidence that they had followed such courses were generally given the preference. This was especially true in the case of appointments in the colleges, some colleges even going so far as to decline to appoint any one who had not taken the degree of doctor of philosophy, which is the badge of the course that involves investigation. As the demand for those who had received this training increased, the number of those seeking it increased at least in the same proportion. New universities were established and old ones caught the spirit of the new movement until from one end of the country to the other centers of scientific activity are now found, and the amount of research work that is done is enormous compared with what was done twenty-five or thirty years ago. Many of those who get a taste of the work of investigation become fascinated by it and are anxious to devote their lives to it. At present, with the facilities for such work available, it seems probable that most of those who have a strong desire and the necessary industry and ability to follow it find their opportunity somewhere. There is little danger of our losing a genius or even one with fair talent. The world is on the lookout for them. The demand for those who can do good research work is greater than the supply. To be sure the rewards are not as a rule as great as those that are likely to be won by the ablest members of some other professions and occupations, and as long as this condition of affairs continues to exist there will not be as many men of the highest intellectual order engaged in this work as we should like to see. On the other hand, when we consider the great progress that has been made during the last twenty-five years or so, we have every reason to take a cheerful view of the future. If as much progress should be made in the next quarter century, we shall, to say the least, be able to compete with the foremost nations of the world in scientific investigation. In my opinion this progress is largely dependent upon the development of our universities. Without the opportunities for training in the methods of scientific investigation there will be but few investigators. It is necessary to have a large number in order that the principle of selection may operate. In this line of work as in others, many are called, but few are chosen.

Another fact that is working advantageously to increase the amount of scientific research done in this country is the support given by the government in its different scientific bureaus. The Geological Survey, the Department of Agriculture, the Coast and Geodetic Survey, the National Bureau of Standards and other departments are carrying on a large amount of excellent scientific work, and thus helping most efficiently to spread the scientific spirit throughout the land.

Finally, two exceedingly interesting experiments in the way of encouraging scientific investigation are now attracting the attention of the world. I mean, of course, the Carnegie Institution, with its endowment of $10,000,000, and the Rockefeller Institute, devoted to investigations in the field of medicine, which will no doubt be adequately endowed. It is too early to express an opinion in regard to the influence of these great foundations upon the progress of scientific investigation. As both will make possible the carrying out of many investigations that would otherwise probably not be carried out, the chances of achieving valuable results will be increased. The danger is that those who are responsible for the management of the funds will be disappointed that the results are not at once of a striking character, and that they will be tempted to change the method of applying the money

before those who are using it have had a fair chance. But we who are on the outside know little of the plans of those who are inside. All signs indicate that they are making an earnest effort to solve an exceedingly difficult problem, and all who have the opportunity should do everything in their power to aid them.

In the changes which have been brought about in the condition of science in this country since 1848, it is safe to say that this association has either directly or indirectly played a leading part. It is certain that for the labors of scientific men increased facilities and a wider usefulness have been procured.

IRA REMSEN.

---

*THE TWENTIETH CENTURY BOTANY.**

AT previous meetings of this and kindred societies the retrospective field in botany has been pretty thoroughly covered. It would seem a fitting time, therefore, to take a glance into the future and endeavor to see what there is for botany and botanical science in the years immediately before us. It is realized that an endeavor to set forth the lines along which botany will develop is a risky thing, and no doubt fifty years hence the views I may express at this time will cause only a smile in the light of actual developments. Notwithstanding this fact, I am willing to essay somewhat the rôle of a prophet, not so much with the idea that I expect all of my prophecies to be realized, but rather in recognition of a principle that to wish a thing or to desire a thing is at least a point gained in the full realization of the wish or desire. What I have to say, therefore, will be rather in the nature of an expression as to what I desire to see brought about in a field of work which to me seems fast opening to great possibilities. If an expression of these desires and the vitalizing of the thoughts which inspire them by placing them before you serve but to put in motion some of the forces which will act for the betterment of botany, my object shall have been fulfilled.

Before taking up specifically the more important lines along which botany seems likely to develop, and before considering some of the demands which may be made upon botany in the twentieth century, I should like briefly to call attention to what may be termed the present attitude of the state toward the work, for about this question hinge some points which are of vital importance to the future expansion and growth of botany as a whole. By the attitude of the state I of course mean the attitude of the people, for, in this country at least, the state is the people. It requires no argument to prove that the attitude of the state toward botany is rapidly changing. Even those of the younger generation realize that within their time the feeling of the people toward botany as a science and botany applied has changed greatly for the good of the work. I believe this is due to the fact that the utilitarian side of botany has been kept largely in the foreground, and the people have come to know and understand that a substantial encouragement of the work means a direct benefit to many important interests. When botany and botanical work were confined largely to the collecting and mounting of plants, the building up of herbariums and, perhaps, the working out of obscure laboratory problems, public sentiment could not be aroused in its behalf. Every time we have reached into new fields with the object of broadening the work and benefiting the people, the people have responded and given us most generous aid.

As an object lesson in this field I may call attention to the rapid growth of botany and botanical work in the Department of Agriculture at Washington. Fifteen years

* Address of the past-president, Botanical Society of America, St. Louis meeting, 1903.

ago the total amount expended for work of this kind did not reach $25,000 annually. The present year the honorable secretary's estimates for the work will aggregate about $400,000; and if the allied lines of investigation in which botany and botanical science play an important part are considered, the funds devoted to the work will exceed half a million dollars. This amount, it must be borne in mind, is an annual expenditure and practically represents an endowment on a three-per-cent. basis of over fifteen million dollars. This is for investigations and experiments alone, as purely educational subjects are considered only in an indirect way. That the people, or the state, are not averse to responding to the needs of botany from the educational point of view is manifested in the remarkable development of the work in a number of our important universities and in the growth of educational institutions, a type of which is found in the New York Botanical Garden. Here, through the energy of a corps of earnest workers, the educational value of botany has been recognized and generous support has been secured for the development of gardens, museums and laboratories. These results, however, I imagine, would not have been attained without appealing to the utilitarian ends in view. The practical value of such an institution to the community and to the country has been presented in the proper way, and the necessary support was forthcoming.

The argument, therefore, in all this is that for the future development of botany and botanical work we must make up our minds to two important things; first, the presentation of our wants to those upon whom we must depend for support, in such a way that the ultimate practical value of what we desire to do will be seen; second, the thorough discharge of our duties to the end of showing that the trust imposed on us has been fully and honestly respected. I may be preaching an heretical doctrine and be criticized on the ground that science has nothing to do with such material things and will take care of itself if kept pure and undefiled. This may be true, but I have long since reached the opinion that the doctrine of science for science's sake may be beautiful in theory, but faulty in practice. Some one has said that pure science and science applied are like abstract and practical Christianity, both beautiful, but one is for gods and the other for men.

It is men that we are to deal with in the future—keen, practical, analytical men, and they want and should know the why and the wherefore of what they are asked to support. It is recognized that there are but few men who have the gift of presenting what is frequently an abstruse problem in such a way as to gain material support. There ought to be more such men, and as the needs of the work develop, doubtless there will be more. From the tendency of the times the fact becomes evident that more and more the pursuit of science must be looked upon in a business-like way. Therefore, future aid for this work, be it in botanical or other lines, must come by going after it in the proper manner. In other words, the scientific man can not afford to wrap about himself a mantle of false dignity and assume that because his work is scientific he is debarred from seeking aid where aid is needed. What we shall expect to see, therefore, in the future is a manifestation of that spirit of progress which recognizes that science must seek its own interests and not wait to be sought.

Science, and I mean, of course, in the main, botanical science, can not and will not suffer by this attitude. I do not mean that the spirit of commercialism, of barter and trade, will enter into the matter. This is an extreme which will be avoided, as well as that other which comes with it, the idea that the responsible head of scientific work

must stand on a pinnacle and say, 'I am a scientist; this is enough; walk up and lay at my feet your tributes in order that you may receive my beneficent smiles.' I am not overdrawing this picture, for this very day there are institutions founded and conducted for the advancement of science where this attitude is maintained. The result is that men with the love of their work at heart who are forced to work under these conditions find themselves handicapped on every side by a sort of immaculateness, perhaps beautiful in theory, but of no practical value in the every-day affairs of life. Under this system work is carried to a certain point, and then, when a little effort would make it complete, the dignity—and I use this word with a question mark—of science looms up, and the needed support must give way to that. Fortunately, botany has not suffered so much from this attitude as some of the kindred sciences, but her cause has been delayed by it in certain cases and is being delayed even to-day.

I repeat, therefore, that the twentieth century shall see this spirit disappear, and in its place shall come one which is fully progressive, recognizing that to be a scientist is to be a man of affairs, a man gifted with that most uncommon of all things—common sense. It will be recognized that 'true science is an invention, the invention of a tool, which will enable man to become more vital, more effective, more adequate in the world in which he finds himself.' This is especially true of botanical science, which in the future must necessarily spread into many walks of life.

It is evident from what has been said that botanists themselves will have much to do with shaping the future attitude of the state toward the work in question. Expediency in all cases will govern the action of the state, and the fact that the botany of the future will more and more become closely identified with utilitarian projects will make the state dependent upon it. The rapid changes taking place in population, the filling up of sparsely settled regions, the shifting of general commercial centers, and the unification of commerce in all its branches will bring more and more imperative demands for plants and their products. With these demands will come the necessity for knowing more of such plants, how to use them to the best advantage, and how to increase the possibilities of production so as to meet the demands of the times. These great questions will necessarily force themselves upon the attention of the state through the demands of the people, and the state will on its part require of those charged with this important work investigations which must necessarily be far-reaching in their importance.

The shaping of these lines of work will, as already pointed out, depend in large measure upon the wisdom and farsightedness of botanists themselves. The fact will not be lost sight of that to attain the highest results the true spirit of scientific work must be kept constantly in the foreground. I maintain that this can always be done in such a way as to command the respect and confidence of the scientific world and at the same time secure the practical aid which must necessarily be at hand if anything is to be accomplished at all. So much, therefore, for the probable future attitude of the state toward botany and botanical science. The high place which botany and botanical work have taken in the affairs of nations during the past few years makes it evident that in the years to come this position will not only be maintained, but materially advanced in numerous directions.

And now let us turn to another somewhat general question which it seems to me must necessarily receive careful consideration in the near future, and that is the effect of the present tendency to extreme specialization in botany. No one, I

think, will question the value of a division of labor in science as well as in other pursuits, but the danger comes from carrying this division too far. The specialist is likely to be a dreamer, and a dreamer is dangerous. He is apt to see things of his own creation and not as they actually exist. I have been fortunate in being placed where I could study the specialist, and while I can not help but admire and encourage the patience and persistency with which a special problem is pursued, I am confronted every day with the fact that a concentration of mind on one subject is apt to distort the vision and bring on a sort of neurasthenia, difficult to combat and wholly unaffected by argument. Now there is danger in this sort of thing, not so much where the specialist is surrounded by other specialists, for here each will have a tendency to dehypnotize the other, if I may use such an expression. The difficulty comes where the specialist is necessarily much alone, where he will not be subject to rude awakenings which will come if his work is under the eye of others. Just as the present tendency in political economy is toward a temporary division of labor rather than a permanent division, so it must be with specialization in botany. From all the signs specialization has reached its extreme development, as is evidenced by the fact that we are beginning to realize something of its dangers. In the near future, therefore, we may expect to see a movement toward better unification of the many special lines of botanical work. Rather than division there will be integration where imaginary lines which have been built up will come down and unification will follow.

When we come to consider carefully some of the effects of specialization during the past few years, we are led to the conclusion that it has had more or less of a tendency to cause working botanists to group themselves into castes. Like other castes, these sometimes look upon each other with more or less respect, and again with more or less disdain. In other words, the tendency to concentrate one's effort on a special subject naturally has a tendency to develop more or less egotistical and conceited ideas as to the importance and value of such subjects. Hence, there is produced a sort of aristocracy which prevails more pronouncedly in some cases than in others. For example, the cytologist is pretty apt to look with more or less commiseration on what he considers his less fortunate brother who may be working just outside the range of the plant cell. Then again, the worker who has branched off into some special morphological line, systematic line or physiological line, even though these may be broad branches of botanical science, considers that his particular field is naturally preeminent, and that in handling his problems he must do so without full regard for the consideration of all the questions involved in the other problems. No one can question the fact that specialization has been of great value, particularly during recent years. It has emphasized the importance and necessity for a concentration of energy in one direction. While this is true, experience has shown, as already pointed out, that such concentration necessarily limits one's field of vision, and as a result the true facts, and especially their relationships, can not always be determined. The reaction against this feeling, which is just beginning to be noticeable, is due no doubt to the gradual realization of the fact that all scientific problems are more or less interdependent. We are coming more and more to see that not only are scientific problems in a particular field interdependent, but that all lines of science are closely related, and that to consider them in the most intelligent and far-reaching manner they must be looked upon as part and parcel of one great whole.

Hence, we look to the twentieth century for material changes in this matter of special work and special problems. There will be closer relationships established in the various lines of investigation, not only so far as concerns different phases of botanical work, but other branches of science as well.

Brief reference has already been made to the educational advances which are likely to be made in botany. But these were educational advances of an indirect sort, which naturally arose out of, or in connection with, pure research. Of course all work is educational, but in the sense that we now use the term we mean work that will in the future be conducted in our schools, universities and colleges. In the light of the developments in this field during the past twenty-five years it would seem hazardous to predict what the future is likely to bring forth. Twenty-five years ago the subject of botany in any of our best educational institutions meant primarily teaching in systematic botany. Naturally, the bringing together, grouping and naming of our more or less virgin flora attracted first consideration. Thus systematic botany received an impetus which it maintained for a considerable time. The weakness of the work, however, was to be found in the fact that the problems dealt with had little to do with living subjects. Plants were gathered, named, mounted and placed in herbariums, and the whole question of proper relationships was based on unsound and fallacious reasoning. Naturally, the paramount question here was one of names, and we are still struggling in a maze of doubts and uncertainties which are the direct outcome of our efforts to correct what appeared to be a growing evil.

Perfection, however, is never reached in a leap. Human nature must have experience to guide it, so that we must look upon all that has been done in the past in the matter of systematic work as essential to broader views and broader aims for the future. It is believed, therefore, that systematic botany in the twentieth century will take on new strength as a result of an increasing study of living plants and a better understanding of the manner in which species come into existence. The complicated problem of species relationships will no longer be a matter of more or less guesswork, but will be considered in the light of the results of actual experimentation with the plants themselves.

In this connection the question of meeting some of the requirements for study in this and allied fields will have to be considered. The experience of the old world in the matter of botanic gardens is such as would suggest caution in any attempt to emulate what has been accomplished there. Representative collections of living plants are highly important and valuable, but in bringing them together the fact should not be lost sight of that botany can in the future be advanced by giving more heed to the esthetic side of the work than has been done in the past; that is, assuming that collections of living plants are for study and general educational effect, much of their value in both directions may be lost by adhering too closely to rigid systems. Collections meeting every requirement for study and having great value in a general educational way will probably be maintained in what is more likely to be a natural system. Such collections can, moreover, be maintained at much less expense than the stereotyped ones, and will do much to bring the science of botany home to large numbers of people who can appreciate a bit of lovely landscape, but can see nothing in the little plots and formal labels so suggestive of cemeteries. In other words, it seems to me that the old idea of botanical collections, with small groups of plants representing certain systems of botanical

nomenclature or certain systems of botanical grouping, will give place to natural gardens where may be grouped herbaceous, shrubby and other plants in such a way as to appeal to the mind through the eye. Unquestionably a much greater appreciation of botany and botanical work can be brought about by gardens of this kind, and it is believed that great encouragement will be made in the matter of their development at educational institutions wherever opportunity affords.

In morphology and physiology we shall expect to see more and more important problems worked out by experimental methods. Less attention will be given to the mere accumulation of facts without proper coordination. The value and importance of experimental morphology are already beginning to be realized; that is, experimental morphology from the standpoint of work on plants in their natural environment rather than under laboratory conditions. The same is true of physiology. In the past our knowledge of plant physiology has been largely based on laboratory work and studies of one or more individual plants. From such data broad generalizations have been made, which, as time has shown, have in many cases been erroneous. In other words, it has been found unsafe and unreliable to base generalizations in the matter of the life processes of plants on laboratory experiments alone. The physiology of the future will undoubtedly pay more heed to the broader questions of plant life in their relation to environment and their adaptation in general to surrounding conditions. In other words, ecology in its broad sense is to be an important factor in the future study of plants. In the past we have had a school of scientific workers arise and endeavor to demonstrate that the growth of plants is controlled in large measure by the chemical properties of the soil. More recently another school has developed in which the physical properties of the soil are pointed out as the chief factors in influencing life processes. Those who study plants themselves can not accept such generalities. It is not safe. Future ecological studies will undoubtedly furnish much new light on the true relationships existing between plants and their environment. These questions must naturally receive a great deal of attention for the reason that many of the most important problems in agriculture, horticulture and forestry will be based upon them.

It is in pathology that we shall expect to see very important advances within the near future. This science is just on the threshold of its development. From the purely utilitarian standpoint it will be of vital consequence, and everything in the nature of strengthening it will necessarily need to receive most careful thought. The pathology of the future will have its groundwork in physiology. Less and less attention will undoubtedly be given to the mere question of remedial measures, and more thought will be paid to the causes of plant diseases and the relation of environment to these causes. The highest type of pathological work, in other words, will be in the field of preventive measures, either by the correction of unfavorable conditions or by developing plants in such a way that they can meet conditions which are not favorable.

In the light of these probable developments, an important question to consider is: Where are the workers to come from and how are they to be trained? Undoubtedly in the future much greater interest will be taken in botanical work in our educational institutions, for the reason that it is gradually coming to the knowledge of young men that there is a demand for persons well trained in plant lines. As a matter of fact, during the last few years the supply of

such men has not been equal to the demand. The reason for this is not far to seek, for there still exists in the minds of most young men who go to college an idea that their future welfare in large measure depends on taking some academic course. It seems important and necessary, therefore, that botanists should put forth their best efforts to bring about a better appreciation of the advantages to be gained in the field of botanical work. A number of colleges and universities already have courses of study which pretty well equip graduates for the advanced work in botany now required. In the future there will be more, and at the same time there will be a greater encouragement for applied work than there is at present. In most colleges it is not practicable at the present time to give men the necessary training for government work. A few years ago this was different, for at that time a graduate from one of our best universities was able at once to meet the general requirements of government investigations. The government requirements, however, have been broadened, so that men capable of handling the problems which now present themselves must necessarily have some preliminary experience with men and affairs before they are in a position to take up independent problems. With a good foundation training in botanical science, especially plant physiology and pathology, a good training in languages and a proper appreciation of the relation of science to practice, men can soon get a sufficient grasp of broad problems to make themselves exceedingly valuable. Those who from temperament or for other reasons are interested only in pure science must necessarily have their field of work limited. For this reason it is believed that in the future colleges will more and more endeavor to emphasize the value and importance of applied work.

After reviewing, necessarily with more or less haste, these various questions as to the probable future development of botany, I have left for the last the question which seems to be of primary importance, for upon a proper appreciation of it will depend much of the success of whatever is undertaken in botanical lines during the years to come. I refer to the necessity for properly organizing the botanical forces which not only exist now, but which are likely to come into existence as the years go by. We have developed in this country a group of botanical organizations, all of which are doing good work and most of which have arisen largely out of the exigencies of the moment. There has as yet been little attempt toward a proper coordination of these various forces, with the object of bringing about unity of action upon all matters which will be for the best interest of botany and botanical work in the broadest sense of the word.

This society was organized primarily to take the lead in botanical work in America. Its standards are high and should be maintained. Criticism, if criticism may be offered, of the work of the society, is that it has so far not developed the individuality that might have been developed, in the light of the questions which were in mind at the time of its organization. The papers which are offered do not differ materially from those presented by other societies and organizations. To my mind it has not been so much a question of the presentation of papers as some would think. Unless the papers presented can be in some way made different from those offered in other organizations, there is little to be gained by presenting them except affording an opportunity for those who wish to bring their problems before coworkers. It would seem to me that this society might very well dispense with a considerable portion of this plan, and devote its energies more in the future to broad questions of shaping policy

in botanical work generally throughout the country. To accomplish this, it is realized that the aid and cooperation of all other botanical societies should be secured. No question is raised as to the value and necessity of other botanical organizations. We do not believe that there are too many of them, but that there is a woeful lack of proper unification and coordination was shown at the last Washington meeting, where the number of papers presented was so great that it was impossible for visiting botanists to take anything like advantage of them. In the future it is hoped and believed that existing botanical organizations can be continued and their integrity and independence maintained, but at the same time it would seem highly important that some steps be taken toward unification. There would seem no reason why the Botanical Society of America should not be the medium for bringing this about, and why, through its efforts, there should not be effected an organization representing the various botanical societies throughout the country which would affiliate with this society and assist in shaping a general policy on all matters affecting the welfare of the science.

The time seems ripe for bringing about this result. Never was botany more prosperous, never more aggressive. On the threshold of the twentieth century we stand, knowing our strength and only needing to weld it into harmonious action to make it vital and lasting. Let us join hands and do our best to bring this about.

Beverly T. Galloway.

*VITALISM AND MECHANISM IN BIOLOGY AND MEDICINE.**

Until some sixty years ago the prevalent view was that nearly all life phenomena were guided essentially by an all-pervading vital force. Even after the discovery by Wöhler in 1828 of the possibility of producing synthetically such an organic substance as urea, such a universal mind as that of Johannes Müller was still clinging to the belief in the all-powerful force as the creator and harmonizer of the various mechanisms of the living body. The belief in the omnipresence of an all-creating vital force furnished little stimulus for laborious studies of the innumerable mechanisms of life. In the forties of the last century, however, there came a change. With the improvement of the methods of investigation, with the rapid progress in organic chemistry, with the establishment of the law of conservation of energy in physics, with the successful application of physical and chemical laws to some of the intricate problems of life, the conviction developed that a great many of the mysteries of life will resolve themselves into physics and chemistry, and this belief gradually grew in some quarters into a theory that all life phenomena are nothing else but complex phenomena of the inorganic world. As just in those days it was recognized in physics that all energies can be converted into motion, and that the mechanical energy is the essential principle in the inorganic world, the new theory which made no distinction between the animate and inanimate phenomena became known as the mechanical theory of life. Right or wrong, this theory was of incalculable benefit to the progress of the biological sciences. The conviction that all parts of life are accessible to an analysis by the methods employed in natural science, stimulated then and stimulates now thousands of patient investigators in their indefatigable attempts to unravel an infinitely small fraction of the mysteries of life. Vitalism had a paralyzing effect. The mechanical conception of the life phenomena

* Introductory remarks made at the D. W. Harrington lectures on 'Œdema, a Consideration of the Physiological and Pathological Factors Concerned in its Formation,' delivered at the University of Buffalo, November 30, December 1, 2 and 3, 1903.

as a working hypothesis is a marvelous stimulus. But it did not remain a working hypothesis.

Men of letters with a transcendental bent of mind have turned it soon into a philosophical system and have extended it to regions which can never become the domain of natural science. Some of the extravagances proclaimed in the name of the mechanical theory brought undeserved discredit upon it. I need only to remind you of the statement that ideas are secreted by the nerve cells just as urine is secreted by the kidney epithelium. Assertions of this kind initiated a reaction against the entire theory. The theory of natural selection by Darwin, which, during its rise, lent its glory to our theory, since in the minds of the literary public the two were naturally linked together, subsequently also brought some discredit to it during its slow descent in the favor of that public. Furthermore, the very incessant activity in the investigation of biological problems which was stimulated by the mechanical theory soon brought out the unmistakable fact that, so far, comparatively only a small fraction of life phenomena are accessible to interpretation by the physics and chemistry of our day, and the enthusiastic originators of the mechanical theory have inadvertently proclaimed that the physics and chemistry of their day would explain all life phenomena. What a failure! say now the growing number of vitalists, or 'neo vitalists,' as they choose to call themselves. Since the middle of the eighties of the last century a reaction set in against the mechanical theory. In all branches of biology an increasing number of writers of first standing are coming out, veiled or open, against the mechanical theory of life. We meet them in physiological chemistry, in general biology, and we meet them in the writings on medicine, the science as well as the practice of medicine. We meet them in the writings on the very subjects I am going to discuss before you, on the subjects of the production of lymph and formation of œdema. And withal the vitalism of our day is not such a modest or conservative creation as the prefix 'neo' would lead us to believe. For instance, because only certain substances are absorbed within the intestines, a selection that can not be explained by the laws of diffusion and osmosis as we know them to-day, it is assumed by some writers that the epithelium of the intestinal mucosa has a selective power. But, instead of considering this assumption merely as a temporary resting place, until we know something more of physics and chemistry, the conclusion is drawn by Neumeister, a distinguished physiological chemist, that the epithelium possesses as much sensation, as much judicial power to know what is good for the body, as the nerve cells of the cortex. In what essential respect does this statement differ from the one of Carl Vogt, which was quoted above and which had such a shocking effect upon his contemporaries, namely, that there is not one difference between the nerve cells which secrete ideas and the kidney epithelium which excretes urine?

The point is that Vogt as well as Neumeister, though both excellent scientists, have not made their assertions as naturalists but as philosophers, who are dealing with transcendental problems. The discussion which is going on between the vitalists and mechanists and which has not only a theoretical but also a very important practical bearing upon many problems in biology and medicine, suffers, in my opinion, from a confusion of conceptions with regard to the questions to be answered. Permit me to discuss here the problems of vitalism and mechanism from my own point of view.

The phenomena of life are apparently different from those of the inorganic world.

We wish to recognize as much of them as our human faculties will permit, and wish to study them by methods of investigation which proved to be reliable in the investigations of the phenomena of the inorganic world. Then there are some preliminary questions to be answered.

TRANSCENDENTAL VITALISM AND MECHANISM.

The first question is: Suppose there will come a time when all laws of the inorganic world and also all structures and laws of the animated world, as far as they are accessible to the human faculties, will be completely known—will it then be found that the phenomena of life can be completely solved, or will it be found that life has still an element which is inconceivable, inaccessible to the grasp of human faculties. This is the concise question between mechanism and vitalism. What should be our position with regard to that question? To this I say it is wholly a transcendental question and not one for physiology and biology to deal with. Since from the point of view of the natural or rather biological sciences we wish to investigate only that which is accessible to human faculties and by methods approved in the natural sciences, we can obviously have no scientific opinions on a subject which is admittedly above the human faculties. An answer in the mechanistic sense is not a whit more scientific than an answer in favor of vitalism would be.

This position, however, should not be interpreted as denying the right to entertain such a question. It is certainly a perfectly legitimate problem in pure philosophy. Neither do I mean to deny the naturalist the right to discuss philosophical problems. But in such a case the discussion in both domains ought to be carried on strictly separately, otherwise, as experience teaches us, a harmful confusion will be unavoidable.

To repeat again, we consider the problem formulated in the preceding question as a transcendental one, and we shall, therefore, designate the theories contained in the answers to it as transcendental vitalism or transcendental mechanism.

NATURAL VITALISM AND MECHANISM.

The second question is: Suppose there shall come a time when all laws of the inorganic world as well as the structures and laws of the animated world shall be perfectly known to us. Would it then be found that the animated world is governed exactly by the same laws as the inanimate one, *i. e.*, by the laws of physics and chemistry, as they will then be known; or will it be found that the vital phenomena, in addition to the chemicophysical forces, are pervaded by separate energies, separate forces which are specific for living matter? It must be admitted that this question is a perfectly legitimate one and within the bounds of natural science. It is perfectly conceivable that one group of natural phenomena might possess energies which other groups do not possess and that vital phenomena might differ, indeed, from the phenomena of the inorganic world by a plus of specific energies. In contradistinction to the transcendental theories of life we might designate the theories contained in the answers to the second question as natural mechanism and natural vitalism. In other words, then, the theory of natural mechanism assumes that all the conceivable laws of life will prove to be nothing but physics and chemistry, and the theory of natural vitalism assumes that all vital phenomena are directed by specific energies besides those which are found also in the physical world. A little consideration will show that the natural and transcendental theories are perfectly independent of one another. For instance, the transcendental vitalist can easily accept the

theory of natural mechanism, and the defender of the theory of natural vitalism may accept the theory of transcendental mechanism. I shall, however, certainly not dwell here on the particulars of this point.

What shall be our position with regard to the problem involved in the second question? It seems to me that the state of our present knowledge does not permit us yet to decide the question in one way or another with any degree of probability, and that for a great many years to come any decision of this problem will have to be considered as an arbitrary hypothesis without a sufficient scientific basis. The argument in favor of vitalism, brought forward recently by Bunge, Neumeister, Stacke, Kassowitz and many others, consists in the statement that the further the investigation in biology progresses, the more facts are brought to light which can not be explained by physics and chemistry. But what does this signify? Our present knowledge of physics and chemistry surely is a most minute fraction of that which we shall know of the laws of the inorganic world in the thousands of years to come. Considering the length of human history we have to admit that even the science of physics is only in its very infancy. Why, it is only recently that they have tortured the father of physics for stating that the earth is turning around the sun, because it hurt their feelings to acknowledge that the abode of man is not the center of the universe. And it hurts the feelings of men to be told that the mysteries of life are only unrecognized chemistry—hence the passionate crusade in some circles against mechanism in biology. In our very day undreamt of discoveries are made in physics and in chemistry. Think of the rays discovered by Roentgen which penetrate heart and kidneys. Think of the marvelous results of stereochemistry, of the laws of osmosis, of the ionization of solutions, etc., all discoveries of our time. Why should we already now positively deny the possibility that chemistry and physics might not finally elucidate a great many, and perhaps all the facts in biology? Furthermore, the attempts properly and systematically to apply physics and chemistry to the interpretation of biological phenomena are hardly older than half a century. Those among the crusaders who themselves lent a hand to such studies should know with what immense difficulties the physiologist has to struggle. He has to create his own physics and chemistry; he has to master a difficult and difficile technique, and then the difficulties in obtaining and handling living material. The physicist and the chemist had always the aid of gold-seeking people. There is no gold for physiology, but plenty of obstruction on its onward way, placed by the sentimentalist, the ignorant and the wicked. With all the obstacles, physiology has already succeeded, in a great measure, to apply physics and chemistry to a good many biological phenomena, and the outlook for the future is brighter than ever. Think of the astonishing discovery in our country by Jacques Loeb of artificial parthenogenesis by simple changes in the osmotic pressure in the surrounding medium of the ovum, a fact which was never dreamt of before!

No, the crusaders against mechanism are wrong in their pessimistic views. There is nothing in the present stage of our knowledge discouraging for the hopes of those who believe in the ultimate solution of the problems of vital phenomena by the physics and chemistry of a far-off future. But it is also true that the success attained at present is, in comparison with what has yet to be attained, too minute, too insignificant to justify a prediction with any degree of probability.

Transcendental mechanism and vitalism

have no place within the domain of natural science. Natural mechanism and vitalism are insufficiently supported by accumulated evidence to be considered as well-established scientific theories.

VITALISM AS A WORKING HYPOTHESIS.

But there is still another question. There are already numerous well-established biological facts which can not be explained for the present by physics and chemistry, and we have no means of knowing whether they will ever be explained that way—what are we to do with these facts? Here is the answer: Vitalism as a storage place is indispensable. We should continue to call these facts vital phenomena until we discover a way to explain them by laws governing the inanimate bodies. But I shall still go further. I believe that vitalism as a working hypothesis is of great advantage to the progress of biology. The belief that only those biological facts which can be reduced to physics and chemistry can be considered as scientifically understood, combined with the misleading and harmful notion to elevate physiology to an exact science, confined the activity of this biologic division to some favored domains—to its own detriment. The sterility of some parts of physiology is due to this inappropriate exclusiveness. The relation of the internal secretion of the thyroid to myxœdema and cretinism and of the pancreas to diabetes, was discovered without any reference to physics and chemistry and was discovered by medical men, and not by physiologists. The important fact of the marvelous effect of the extract of the suprarenal capsule upon the circulation was discovered by physiologists without any reference to physics and chemistry. Surely physiology ought to search for the physics and chemistry of the vital processes as much as possible, but it ought to do more. It ought to unearth vital phenomena, study their characters by methods peculiar to themselves, and establish their laws aside from any relation to physics and chemistry of the inorganic world. That this can be successfully done is shown by the marvelous results obtained in the discoveries and the precise studies of toxines, antitoxines, hæmolysines, cytolysines and their like without much regard for physics and chemistry. Especially medical men have reason to ask for such physiological studies. The experiments which nature is continually making upon human beings and which physicians are called upon to interpret and to mend are not confined to domains which are accessible to interpretations by physics and chemistry. And it is to such a far-seeing, liberal, broad physiology that the science and practice of medicine is looking for a delivery from the firm grasp of the one-sided teachings of pathological anatomy.

S. J. Melzer.

SCIENTIFIC BOOKS.

Mammalian Anatomy, with special reference to the Cat. By Alvin Davison, Ph.D. Philadelphia, P. Blakiston's Son & Co. 1903. 8vo. Pp. xi + 250; 108 figs.

Another book on the anatomy of the cat can not but awaken suspicion as to its utility, but an examination of this one shows the suspicion to be unfounded. It is designed to fill the gap between the more detailed works and those which are merely laboratory guides, and to afford the student who can not pursue a lengthy course of zoological study, a general idea of the structure of a mammal and of the principles of mammalian anatomy.

In writing such a work the important point is to determine what is to be omitted, and Professor Davison has treated his subject with an admirable perspective. Occasionally, as in the description of the peritoneum, a somewhat fuller development of the subject would have been advisable, and occasionally, also, a brevity of statement tends to convey a somewhat erroneous impression. But such errors

are few and the book furnishes an excellent idea of the structure of the cat, free from a superfluity of detail which too often serves merely to conceal from the young student the fundamental principles which they may be intended to elucidate. Profusion of detail does not always make for accuracy in the student and it is principles rather than facts that he should acquire from his laboratory training.

Throughout the book are frequent remarks of a comparative nature and at the close of each chapter is a list of questions or suggestions, for the most part of a general nature, which will serve as excellent topics for comment by the teacher or for collateral investigation under his direction by the student. An introductory chapter is devoted to an account of useful methods by which the dissection of a mammal may be facilitated, and the text is illustrated by numerous figures and diagrams for the most part admirably executed.

J. P. McM.

SOCIETIES AND ACADEMIES.

PHILOSOPHICAL SOCIETY OF WASHINGTON.

THE 573d meeting was held November 7. Dr. A. L. Day spoke on 'The Black Body and the Measurement of Extreme Temperatures.' He outlined the history of the theoretical study of the problem, and showed how such a body had been constructed artificially; he then discussed at length the results of experiments made with it, pointing out the relation between the temperature and the total radiation, and between the temperature and the wave-length of radiation of maximum intensity, and expressing these relations by equations; from these equations temperatures outside the range of measurement can be calculated by extrapolation.

Mr. C. E. Van Orstrand followed with 'Notes on the Emission Function,' discussing mathematically the second of the equations presented by the preceding speaker.

AT the 574th meeting, held November 21, the subject of 'Synchronous Actions in the Atmospheres of the Sun and the Earth' was discussed by Professor F. H. Bigelow, of the Weather Bureau. The curves first published in 1894, showing simultaneous variations in the sunspot areas, the magnetic field, the pressures and temperatures of. the northwestern states, the movements in latitude and longitudes of the storm centers, were compared with the prominence secular variations and found to agree. The meteorological data have been extended to all parts of the earth and they give similar variations, supplemented by inversion of the type. Thus the direct type of temperature prevails throughout the tropics, and the inverse type in the temperate zones; the direct type of pressures holds around the Indian Ocean and the inverse type in North and South America. The distribution of the prominences in latitude and their movements in the eleven-year cycle were explained, also their distribution in longitude. From the latter were derived the periods of rotation of the sun in different zones, and the variations of the several periods in the eleven-year cycle, which gave the same curve as holds for the prominence frequency. This important phenomenon was referred back to the internal circulation of the sun, and it confirms the second case of von Helmholtz's equations, as applied to a rotating mass heated at the center. The fundamental period of the sun's rotation is that of the equator, 26.68 days, and as this is the shortest possible period in the sun it follows that numerous determinations of the solar rotation from terrestrial phenomena, such as aurora, thunderstorms, must be excluded as misleading. The observed synchronism at the earth has its basis in the sun's circulation, and this is of a kind to produce vertical polarization, and an internal magnetic field. Hence all stars should be magnetized while the process of cooling under their own gravitation is going on.

Mr. L. A. Bauer then presented several brief 'Contributions to the Theory of the Earth's Permanent Magnetism.' He showed that the energy of the earth's magnetization had diminished by one thirty-sixth part in forty-six years. He stated as a result of his analysis that the principal cause of secular variation resides outside the earth's crust. He also attempted a calculation of the magnetic en-

ergy per unit of area at the surface of the earth.

The 575th meeting on December 5 was set apart for the annual address of the retiring president, Professor James Howard Gore. His subject was 'The Geoidal Figure of the Earth.' He pointed out that four views had been held successively regarding the form of the earth—that it was a plane, a sphere, a spheroid, and a geoid; he traced the history of the measurements that had led to the successive views, and discussed at length the present conclusions of geodesists.

Charles K. Wead,
Secretary.

GEOLOGICAL SOCIETY OF WASHINGTON.

At the 146th meeting held on November 25, 1903, the following papers were presented:

Ninth Session of the International Congress of Geologists, at Vienna: S. F. Emmons.

The Alaska-Treadwell Mine: A. C. Spencer.

The Stratigraphic Position of the Judith River Beds: T. W. Stanton and J. B. Hatchel.

The above papers have been or shortly will be published in full.

The 147th meeting of the society was held on December 9. Under the title 'Notes on the Deposition of the Appalachian Pottsville,' Mr. David White presented certain conclusions respecting the physical geography of the Aplachian trough during early Pennsylvanian time, with correlations based largely on the study of the fossil plants. These show the existence in lower Pottsville time of an axial trough near the eastern margin of the present coal region. The loading and subsidence of this relatively narrow trough led to the submergence of the western land, and in late Pottsville time the transgression of the sea across the bituminous regions of Pennsylvania, Ohio, western Maryland and northern West Virginia. The thickness of the Pottsville sediments, about 1,200 feet in the type section, was shown to be about 4,000 feet near the eastern border, in southwest Virginia near the Tennessee line.

Dr. George H. Girty made a comparison of sections of upper Paleozoic rocks in Ohio and northwestern Pennsylvania. He showed that not the Shenango sandstone, as had usually been supposed, but a much lower bed in the Crawford County section, was equivalent to the sub-Olean conglomerate. This was determined by tracing eastward from its typical locality the Corry sandstone, which near Warren was found to occupy a position just above the sub-Olean. The latter, therefore, would appear to occur at about the horizon of the Berea grit of Ohio, which is the same as the Cussewago sandstone, which lies not far below the Corry sandstone in Professor I. C. White's section of Crawford and Erie counties.

The Waverly group of Ohio was explicitly included by Meek and Worthen, along with the Chouteau group of Missouri and the Goniatite limestone of Rockford, Indiana, in their definition of the Kinderhook group or epoch. The only Waverly fauna well known at that time was the fauna of the Cuyahoga shale, and these authors seem to have had in mind as the Kinderhook fauna chiefly that of the Chouteau limestone. If any precise correlation is possible between the Waverly group and the early Mississippian of the Mississippi valley, it lies between the middle member of the Cuyahoga formation and the Chouteau limestone. It follows, therefore, that the series of rocks and faunas in southwestern New York which overlie the true Chemung, inclusive of the sub-Olean conglomerate, recently assigned by Professor J. M. Clarke to the Carboniferous, really lie below the base of the Carboniferous system as at present recognized in this country, just as they lie above the Chemung beds, the recognized top of the Devonian. This series, having an approximate thickness of 500 feet, represents an interval not provided for in the geological time-scale, and for it the term Bradfordian is proposed. This term, which will rank with Senecan, Chautauquan, etc., includes the Cattaraugus, Oswayo and Knapp beds of the New York section, which may provisionally be accepted as its subdivisions. The position of this series as an unrecognized interval in the time-scale is quite apart from the determination of its age as Devonian or Carboniferous,

a question which is reserved for further study and discussion. The Bradfordian faunas are equally distinct from those of the Chemung group, on one hand, and from those of the Waverly group, on the other. They contain to some extent an intermingling of Carboniferous and Devonian species, and are in fact transitional between those of the two eras corresponding to the position of the rocks in which they are found.

A recent bulletin of the U. S. Geological Survey, by Professor H. S. Williams, which deals with the migrations of faunas, so far as it involves the rocks and faunas under consideration, is based upon a misconception of their stratigraphic relations.

This was followed by a paper entitled 'Fluorspar Deposits of Southern Illinois,' by Dr. H. Foster Bain.

These deposits occur within an elliptical area about forty miles in diameter covering positions of southern Illinois and the adjacent part of Kentucky, and forming a truncated dome probably reduced to a peneplain in Tertiary time. The region is one of the normal faulting and the individual blocks of strata are very irregularly disposed. The ore occurs in fissure veins along these fault planes. In the region are a number of dikes of mica-peridotite, biotite-pyroxenite and diabase. The type of deposits, unusual in the Mississippi valley, associated with the igneous rocks suggests a genetic relation and the analogy with the fluorspar deposits of the northern England is very close.

THE 148th regular and 11th annual meeting of the society took place on December 10. The first part of the meeting was occupied by the presidential address of Dr. C. Willard Hayes, entitled 'Should There be a Federal Department of Mines.' Later the reports of the secretaries and treasurers were presented followed by the election of officers for the ensuing year.

President—C. Willard Hayes.

Vice-Presidents—George P. Merrill and Waldemar Lindgren.

Secretaries—Walter C. Mendenhall and Alfred H. Brooks.

Treasurer—George W. Store.

Members of Council at Large—David White, T. W. Stanton, T. Wayland Vaughan, M. R. Campbell and Leslie F. Ransom.

ALFRED H. BROOKS,
Secretary.

CHEMICAL SOCIETY OF WASHINGTON.

THE 144th regular meeting of the Washington Chemical Society was held on October 8, at 8 P.M., in the assembly hall of the Cosmos Club. In the absence of the president, the meeting was called to order by the vice-president, Dr. E. T. Allen.

The program for the evening consisted of two papers. The first paper, entitled 'Second Report on Cement Analysis,' was presented by Dr. W. F. Hillebrand and dealt with the results of the analyses of two samples of cement material, which were made by nineteen chemists working independently. The results obtained by these chemists were compared with a standard analysis made by Dr. Hillebrand and many of the determinations differed very markedly from the standard results. A discussion of these variations was entered into by the author, and it was pointed out that, although many differences existed among the determinations made by the various analysts, it was not necessary to assume that the source of the errors lay with the method, but was due to other factors which must be taken into consideration.

The second paper on the program was presented by Dr. Atherton Seidell, and was entitled 'Precipitation of Zinc by Manganese Peroxide, with especial reference to the Volhard Method of Determining Manganese.' The problem involved and the method used for the analysis of the precipitate formed in the Volhard method for the determination of manganese were briefly described. The results of the investigation lead to the conclusion that zinc is always carried down by the precipitated peroxide of manganese. The amount found in the precipitate depends upon the quantity which is present in the solution at the time the precipitation is made.

The ratio between the zinc oxide and the manganese peroxide found in the precipitates indicates the formation of mixtures having definite molecular ratios.

The precipitate having the composition $4MnO_2.ZnO$ contained the highest relative amount of zinc which could be carried down in combination with manganese peroxide. The water of hydration in the precipitates was found to be variable, and its amount at any of the temperatures selected for drying did not correspond to a whole number of molecules.

The 145th regular meeting of the Washington Chemical Society was held November 12 in the assembly room of the Cosmos Club. Dr. H. N. Stokes and Mr. S. S. Voorhees were elected councillors of the American Chemical Society. Dr. Atherton Seidell was elected secretary of the Washington Chemical Society. The first paper on the program, entitled 'European Notes,' was delivered by Professor F. W. Clarke. The speaker described his recent visit to Manchester, England, in attendance upon the meeting held in honor of the one hundredth anniversary of Dalton's discovery. He also told of his visits to Cambridge and the laboratories of Thorpe and Ramsay in London, to a meeting of the Royal Society and the Royal Society Social Club. A short account of the meeting of the Congress of Applied Chemistry held at Berlin was given, after which he described his subsequent visits to Dresden, then to Munich, where he was shown Beyer's laboratory built by Liebig and also made acquainted with the great work in mineralogy which is now being done by Groth. Dr. Clarke also spoke of his visits to Zurich and to Heidelberg. The second paper, entitled 'The Solubility of Calcium Sulphate in Aqueous Solutions of Sulphuric Acid,' by F. K. Cameron and J. F. Breazeale, was presented by Dr. Cameron. The authors showed that in the presence of any concentration of sulphuric acid the solubility curve for gypsum or calcium sulphate did not show a maximum point, as this substance does in pure water, but increases steadily with increase in temperature. At temperatures from 25° C. to 85° C. the solubility of calcium sulphate increases with increasing concentration of sulphuric acid until a maximum is reached and then decreases again. The position of the maximum point on the curve, the concentration with respect to calcium sulphate and sulphuric acid respectively being taken as ordinates, depends upon the temperature. The data obtained seems to negative the assumption that both electrolytes yield a common ion. These hypotheses suggest themselves:

1. That at higher dilutions sulphuric acid yields mainly an HSO_4 ion and with increasing concentration mainly an SO_4 ion. But this assumption is opposed to the results of previous work of others on the conductivity, etc., of solutions of sulphuric acid.

2. That double or bisulphates are formed. An examination of the solid phase in contact with the solution failed to throw light upon this point.

3. That other solubility effects than that occasioned by the ions masked the action of the latter.

No satisfactory criteria exist by which these assumptions may be adequately tested. The authors do not regard the facts as necessarily opposed to the dissociation hypothesis. But the hypothesis in its present form is unsatisfactory and inadequate to furnish assistance in the study of such phenomena.

It was pointed out that in these solutions there was evidence of a condensation of the solvent, water, which might have an important bearing on the apparently abnormal results. Finally, the solubility of calcium sulphate in pure water was discussed in comparison with the results obtained by other investigators.

A. Seidell,
Secretary.

THE BIOLOGICAL SOCIETY OF WASHINGTON.

The 377th meeting was held Saturday evening, November 28.

H. F. Moore spoke on 'The Artificial Fattening of Oysters,' stating that experiments made by the U. S. Fish Commission showed that when placed in artificial ponds, kept at the right degree of temperature and salinity to foster the growth of diatoms, and with the water kept in motion to imitate the movement of the tide, poor oysters rapidly became fat.

F. H. Hillman described 'The Comparative Effects of the Seed Midge and of *Bruchophagus funebris* on the Structure of Clover Flowers and Fruits.' The speaker stated that

the seed midge, *Cecidomyia leguminicola* Lintner, arrested the growth of the clover corolla, usually causing it to project but slightly from the throat of the calyx, while its base became crustaceous, forming a hardened case about the growing larva. At the same time the pistil became aborted, its growth being arrested before fertilization, while, together with the stamens, it was pushed aside by the growing larva. The attacks of *Bruchophagus funebris* (Howard) does not prevent the complete or nearly complete development of the corolla, which, in this case, does not become crustaceous. The ovary becomes nearly mature, its hardened portion being fully formed. The seed attains nearly full size, but instead of being normally violet or yellow, plump and shining, it is brown, dull and somewhat shrunken. The kernel of the seed is practically exhausted, leaving the seed coat as a frail shell.

These essential differences in the life histories of the insects shown in their effects on the clover flowers and fruits appear to afford conclusive evidence of the correctness of Professor Hopkins's opinion that *Bruchophagus funebris* feeds on the clover seed and is not parasitic on the seed midge, as has been believed.

An examination by the author of 32 red clover heads showed 53 per cent. of the seed farmed to be uninjured, while 47 per cent. was destroyed by the *Bruchophagus*.

Enlarged figures showing the structures discussed were displayed.

Charles Hallock spoke on the subject of 'Sea Trout where no Rivers Are,' the object of the communication being to establish the point that the sea trout is not a fresh-water species with marine habits, but primarily a resident of salt water. It was stated that the sea trout of the Shetland Islands and the Labrador coast, which attain a weight of twenty pounds, do not enter rivers to spawn, nor do more than a moiety of the Canadian sea trout, the bulk of these spawning in estuaries in tide water. While these trout were structurally identical with the fresh-water species, they differed widely from the latter in habits, range, food and appearance, and the speaker considered that these facts should be allowed due weight in differentiating between species. In the course of his remarks Mr. Hallock intimated that the salmon of the Atlantic coast passed the salt-water portion of their life in the subarctic belt, being attracted thither by the abundance of coastwise food.

The fourth paper of the evening, entitled 'The Vegetative Vigor of Hybrids and Mutations,' was read by Mr. O. F. Cook. Hybrids and mutations were interpreted as representing opposite side-paths of the evolutionary thoroughfare, the free interbreeding of numerous moderately diverse individuals being the best condition for evolutionary progress. A declining reproductive power characterizes both of these extreme types of variation, but is often accompanied by unusual vegetative vigor. Physiological and selective explanations of this paradox appear to be inadequate, but from the standpoint of a kinetic theory of evolution it was suggested that the vigor is the same as that of normal variations and crosses, while the relative or complete sterility may be due in both cases to the absence of normal interbreeding, which also induces abrupt variations or aberrations of heredity. Vegetative vigor does not, therefore, conflict with the view that hybrids and mutations are degenerative variations.

F. A. LUCAS.

ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

THE 349th meeting was held on November 3, 1903.

Professor W J McGee gave an account of the work performed by the American Anthropological Association at the recent meeting held in New York and also gave a résumé of the work of the Department of Anthropology of the World's Fair at St. Louis.

Mr. Goddard, of the University of California, was present and was invited to address the society. He told of the investigations being carried on in the language, folk-lore and ceremonials of the Indians of California by the Ethnological and Archeological Survey of the state. He spoke of the extinction of stocks and the decay of customs and urged the aid of students before it is too late.

Dr. John R. Swanton gave a communication

on the Haida and other tribes he has been studying. In his winter field work he hopes to ascertain the relationships, if any, between the Tlinkit and the Haida.

On account of the illness of Dr. Lamb his paper went over, and the society resolved itself into a committee of the whole to discuss the subject of cave exploration.

Professor Holmes stated the problems to be solved and mentioned the explorations of Fowke, McGuire, Putnam and Moorehead. He pointed out that caves show undisturbed sites and hence give a good record, and announced that Professor Putnam has found early man with fossils in caves of California. As yet he said the evidence of early man in the caves exploration in the east is negative.

Dr. Fewkes said that caves were gathering places of men for religious purposes as the Cave of the Sun at Porto Plata, where it is believed by the natives that the sun and moon rose. He stated his belief that the lowest form of man is found in South America and in caves in the region of the Tapuyan stock. Dr. Hrdlicka remarked on the caves of northern Mexico where there are (1) shelters showing evidences of fire, chips and bones, and little art; (2) having human burial; (3) the deep variety containing ceremonial objects, and (4) the dwelling caves, and showed all occur in a region inhabited by a single people.

Mr. McGuire gave an interesting account of his recent cave hunting in Maryland and Pennsylvania. He examined a number of caves, and while the finds were numerous no evidence was found as to the antiquity of man.

Professor McGee said that cave studies should be made not so much for man as for paleontology, and should be a geological problem.

The president told interestingly her observations on cave exploration and said that some Indian words indicate going under the ground to enter the house, and perhaps refer to a period when caves were used as habitations.

The 350th meeting was held November 17, 1903.

Dr. J. Walter Fewkes read a paper on the stone collars and tripointed images, or zemes, of Porto Rico. Doctor Fewkes illustrated his paper with large drawings of the types of these specimens. The collars, which are found almost wholly in Porto Rico, are fine examples of stone working, having gone through the process of carving and polishing after the rough work of pecking with a stone hammer. Each collar has an oval, flat, roughened area on one side.

The tripointed images are of five types: (1) Smooth, without decoration; (2) with conoid projection modified into a head; (3) with face on one side; (4) with head on the right and two legs on the left; (5) with four legs. Most of the images have human faces, though some are in the shape of animals and birds. In reference to the relation between the collars and the tripointed images, Dr. Fewkes called attention to the theory of J. J. Acosta that images generally have the same proportion and were placed on the flat surface of the collar and secured by cords. A specimen showing the feasibility of such junction was displayed. Dr. Fewkes said that there is no proof that these objects are not idols and that they show the representation of anthropomorphic gods in Porto Rico. Most of the collars seem to be serpent forms. In absence of data, however, there are still enigmas that require for their solution more field work and research, to which end Dr. Fewkes will devote this winter's labors in the West Indies.

In answer to an inquiry from the president, Miss Fletcher, Dr. Fewkes said that the triform images are geographical and resemble Yunque Mountain. In answer to a question from Mr. McGuire, Dr. Fewkes said there seem to be more triform images than collars, and he further remarked that the locality where the collars have been found has not been recorded.

The secretary mentioned that Professor Mason had remarked on the similarity between the cedar bark collars of the northwest coast Indians and the stone collars of Porto Rico.

Dr. John R. Swanton said that the resemblance is probably accidental and further said that if the Porto Rican collars were evidence of a serpent cult the art modifications might

be due to the transfer of the cult to a locality where serpents do not exist.

In reference to the human remains collected by Dr. Fewkes, Dr. Hrdlicka said that a comparison of the Porto Rican skull with South American skulls shows it to be like specimens from Brazil. In answer to an inquiry from Dr. Lamb, Dr. Fewkes said the bones were found in a mound near Utuado.

Under the head of voluntary communications, Dr. Hrdlicka suggested that archeological and anthropological work be carried on at Panama in connection with work on the canal.

The secretary presented data on the destruction of ruins in the southwestern United States, and suggested that the movement for their preservation inaugurated some years ago be revived. After a brief discussion in which the president, Dr. Kober, Dr. Fewkes and Dr. Hrdlicka took part, the matter was postponed to the next meeting.

WALTER HOUGH,
Secretary.

DISCUSSION AND CORRESPONDENCE.

THE ANIMAL PARASITE SUPPOSED TO BE THE CAUSE OF YELLOW FEVER.

TO THE EDITOR OF SCIENCE: In your issue of October 23, 1903, you publish a communication from Mr. J. C. Smith, of New Orleans, in regard to the animal parasite in the bodies of mosquitoes infected from yellow-fever subjects. While the article is on its face contradictory and unsatisfactory, its burden is to claim the credit for scientific work to which he is not entitled. It reflects unfairly and unjustly upon Professor George E. Beyer, associate professor of biology in Tulane University, who was the biologist of the working party of the yellow-fever institute of the U. S. Public Health and Marine-Hospital Service, which made the investigations in Vera Cruz in 1902.

Professor Beyer is an acting assistant surgeon in that service, and for that reason can make no publication in the matter.

In the first paragraph of the article Mr. Smith claims that he was 'the first to have correctly interpreted and given value to the things found in the bodies of the mosquitoes infected from yellow-fever patients.' After setting forth this claim, he closes with the vastly more modest claim that he was entitled to have printed in the report of the working party an acknowledgment of his valuable services in working out the sexual life history of the parasite.

Mr. Smith fixes January 23, 1903, as the time when his assistance was asked, and his work was performed subsequent to that date.

The facts are that the working party discovered the animal parasite in mosquitoes infected from yellow-fever subjects in the summer of 1902, that they classified and named the parasite, illustrated it with drawings, and sent the drawings in November, and a preliminary report to the Surgeon-General in July of 1902, nearly six months prior to the time fixed by Mr. Smith. This report is an official record, is on file in Washington, and of itself shows that Mr. Smith was neither the discoverer nor the first correctly to interpret the parasite.

The eighth paragraph of the article does a particular injustice to Professor Beyer. It says: "Up to this time (January 23, 1903) Professor Beyer, who was the biologist of the party, knew of no evidence of a parasite in these mosquitoes, excepting some granular bodies, as they were styled, which were found in the cell of the salivary glands, and which I afterward showed the party were not granular bodies, but were linear bodies, five or six times longer than wide, the sporozites. On January 30 [1903] I reported having found in the bodies of a number of the mosquitoes an animal parasite in process of sexual development."

Professor Beyer had found this parasite six months before the time fixed by Mr. Smith as the day when he saw it in slides loaned him by Professor Beyer and known by the latter to contain the parasite. A number of physicians were acquainted with the discovery, its interpretation and value, in the summer and fall of last year. Dr. N. Del Rio in a statement acknowledged before the American Consul at Vera Cruz, June 8, 1903, says that as delegates of the Superior Board of Health of Vera Cruz, he, Dr. Matienzo and Dr. Iglesias were, during June and July, 1902, shown by Professor Beyer in the stomach and

glands of mosquitoes infected with yellow fever, an animal organism which the members of the American Commission classified as a Protozoan of the order of Coccidiida.

Dr. Henry R. Carter, a distinguished surgeon of the Public Health and Marine-Hospital Service, in a letter dated October 31, 1903, says that while attending the Public Health convention in New Orleans, on December 12, 1902, he visited Professor Beyer's laboratory in Tulane University, with several other physicians, and was shown a number of slides under the microscope. These showed, Professor Beyer told him, sections of the stomach walls, thorax and salivary cells of mosquitoes, with bodies which Professor Beyer claimed were the coccidium, and explained the stages in detail. Dr. Carter says that unquestionably, at that time, Professor Beyer claimed that his slides showed the sexual stages of a coccidium and that he had demonstrated the sexual cycle of a coccidium in the infected *Stygomyia fasciata*.

The proof that the work which Mr. Smith claims to have done in January of this year was all originally done in the summer of last year by the working party of the U. S. Public Health and Marine-Hospital Service is so clear that it is difficult to see how Mr. Smith could set up such a claim. The letter of Dr. Pothier which he prints in his article is contradictory of his claim.

Mr. Smith was consulted in January of this year and corroborated the work already performed. Ratification by a man of his undoubted high scientific knowledge was valuable. Professor Beyer has willingly counseled giving Mr. Smith all due acknowledgment, and has never sought to withhold all that he was entitled to, that is, due recognition of his assistance in demonstrating the life cycle of the parasite.

Mr. Smith has never published any interpretation of the coccidium different from the working party's. It is hard to see, therefore, how he was the first correctly to interpret the discovery when his interpretation was the same as that made by the working party months before.

I ask that you publish this refutation of Mr. Smith's claims in the same manner as his article. This request is made with no wish to provoke a controversy, but solely with a view to correcting an injustice.

I also suggest that a warning note be issued by you against a too hasty conclusion that the animal parasite discovered in infected *Stegomyia fasciata* be accepted as the cause of yellow fever. The working party's report makes no such claim. Surgeon-General Wyman recently issued a letter pointing out that this claim is not made. The value of the discovery of the coccidium lay in the fact that it pointed out a path for future investigation.

H. W. Robinson.

New Orleans,
November 28, 1903.

SHORTER ARTICLES.

THE NEW COSMICAL METEOROLOGY.

With every fresh outburst of large spots on the surface of the sun there is likely to be a sympathetic disturbance in the terrestrial magnetic and electrical fields, a change in the weather conditions of the world, and a recrudescence of popular interest in the subject. Speculation as to the causal connection between this solar action and the terrestrial effect is apt to become extravagant, even going to the length of seeking to identify particular spots on the sun with individual storms on the earth. This procedure overlooks some facts in the chain of events which in reality bind the two phenomena together, and it is the purpose of this paper to present in a somewhat orderly form the sequence as at present understood.

It has been found necessary to include both the sun and the earth in our meteorological research, and properly so, because the atmosphere of the sun is at work in sending energy, and the atmosphere of the earth is receiving energy, each through its process of convection and radiation. By these agencies, a special circulation is sustained in the atmosphere of the sun, and another in that of the earth, and the energy of one passing into the other binds the two together in a single cosmical thermal engine. Solar physics and astrophysics are evidently only other names for meteorology, which embraces all atmos-

pheric phenomena in its scope. The details of the work of the Weather Bureau in this research are being published as rapidly as possible, but as some time must elapse before this will be completed, it may be of interest to make a comprehensive statement of the conclusions that have been reached.

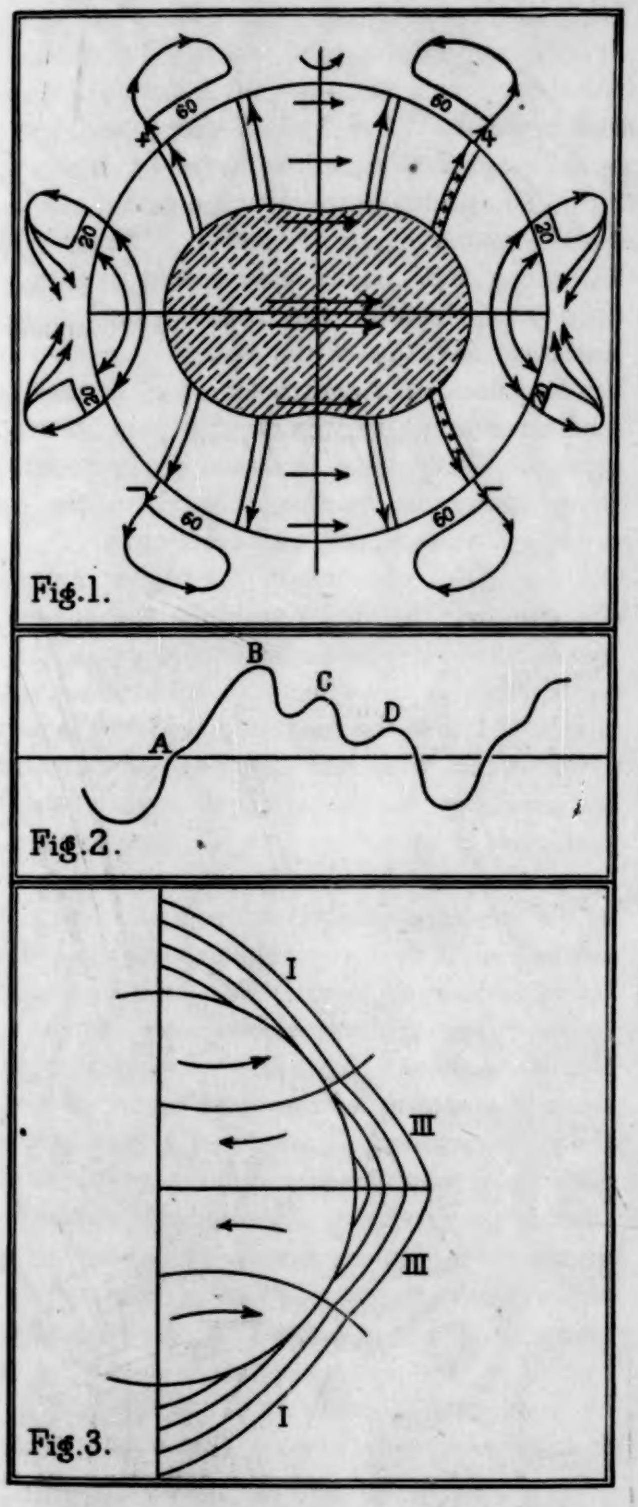

Fig. 1.
Fig. 2.
Fig. 3.

The Circulation of the Sun. (Fig. 1.)—The thermodynamic conditions in the sun suggest a viscous nucleus extending about half its radius from the center, which is surrounded by a gaseous envelope, the sun's proper atmosphere. The nucleus is apparently not spheroidal, but dumbbell in shape, according to the Jacobian ellipsoid of equilibrium, so that the sun is an incipient binary star with two centers of action instead of only one. This result rests upon the following facts: (1) The prominence frequency numbers on the surface have two distinct maxima, which move in opposite directions from the middle latitudes, one from latitude 25° towards the equator, as do the sunspots and faculæ, and the other from latitude 50° towards the poles, in the course of an eleven-year cycle. The cycle begins at minimum with a strong outpouring in middle latitudes, which separates into the two branches mentioned. It is probable that the congested energy of the interior first seeks to escape from the region where the viscous nucleus ends, and that one wave spreads through the gaseous region towards the end of the equator on the surface, while a second wave passes through the nucleus towards the center of the sun. The course of the maxima points, as shown in the *Monthly Weather Review* for January, 1903, favors this explanation. (2) The distribution of the prominences in longitude gives two maxima, located on two opposite meridians of the sun, as if they sprang from two foci. (3) This division of solar activity is also found recorded in the distribution of several other products of solar energy in the period of the solar rotation, which is 26.68 days on the equatorial plane, as in that of the sunspots and the faculæ, in one system of deflecting forces of the terrestrial magnetic field, in the barometric pressures and in the temperatures. That would be a good reason, if it exists, why the sun in its rotation should effect differential impulses throughout the cosmical system.

The periods of rotation of the sun have been determined in the several zones by a discussion of the prominence numbers, and there is retardation from the equator to the poles. This conforms to von Helmholtz's Case II., derived from the general equations of motion,

for discontinuous surfaces of different temperatures sliding past each other with different velocities, and rolling up vortex tubes between them. The layers are warmer around the axis of rotation of the sun, and have slower angular velocities than those more distant from it. The vortex tubes have the shape indicated in Fig. 1, right-handed in the northern hemisphere and left-handed in the southern. If the constituents of rotating matter carry electric charges in their atoms and molecules, this vortex entrainment will produce polarization and a true magnetic field extending outside the sun. The rotation period of the magnetic field near the poles, since it is primarily seated in the nucleus, is the same as that of the surface at the equator, namely, 26.68 days. The earth's normal magnetic field has a component system impressed upon it which is directed from north to south perpendicular to the ecliptic, and these vectors are probably portions of the lines here described as springing from the solar nucleus. Furthermore, all large cooling masses, contracting by their own gravity and rotating on an axis must, in conformity with the equations of motion, set up such a polarized internal structure, and, therefore, all stars are probably magnetic. The earth still possesses a residual magnetism originally produced in this manner, which is gradually fading away as the earth cools, and will become very feeble as the loss of convective heat progresses, somewhat like that of the moon at the present time. The belt systems on the planets Jupiter and Saturn afford examples of rotations with discontinuous surfaces, and minor vortices between them, under this law. The granulated surface of the sun is probably due to this vortex motion, where each granule represents the discharge of a single vortex tube.

The Solar-terrestrial Synchronism. (Fig. 2.)—The eleven-year cyclic period of the sunspot variation gives a curve with one principal maximum and one principal minimum, but this register of the solar action is not so sensitive as that recorded in some of the other elements. The eruptions of the sunspots and the faculæ are confined to the gaseous envelope, and do not directly represent the working of the viscous nucleus. The prominences of higher latitudes, 40° to 80°, produce the same fundamental curve, but there are minor crests superposed upon it, sometimes one on the ascending branch, *A*, and usually two on the descending branch, *C, D*. In some of the eleven-year cycles *A* does not appear, and one might count the length of the short period from the three crests, *B, C, D*, and make it, $11.1 \div 3 = 3.7$ years, as Lockyer has done. I have taken the four crests, *A, B, C, D*, and make the average period, $11.1 \div 4 = 2\frac{3}{4}$ years, as in 'Weather Bulletin' No. 21, page 125. This more sensitive curve registers primarily the action of the solar nucleus, and the minor crests are the recrudescences of a contracting and congesting medium seeking to free itself of supercharged energy. The curve is found to be repeated in a remarkable manner throughout the cosmical system. Thus, we have found, (1) that the periods of rotation in the higher zones of the sun, 50° to 70°, reproduce the curve in a secular variation, and refer its cause, without doubt, to the effects of internal circulation;* (2) that the magnetic field at the earth synchronizes with it;† (3) that the terrestrial temperatures in the tropical zones give the same curve directly, but in the temperate zones they synchronize in an inverted form; while the terrestrial pressures synchronize directly with it in the regions around the Indian Ocean, Australia, South Asia and Africa, but in an inverted form throughout North and South America. This inversion implies that there is a surging of the earth's atmosphere in the process of its general circulation, whereby a portion rises in pressure and temperature while another portion falls. This opens up a new field of meteorological research. A laboratory experiment, by means of cathode rays within a magnetic field, matches the observed distribution of the solar corona, and this is also in harmony with analysis of the sun's physical condition here outlined. The computed system of ordinary magnetic deflecting vectors and of the large magnetic storms which disturb the earth's normal field and fluctuate in

* See *Monthly Weather Review* for October, 1903.

† See *Monthly Weather Review* for July, 1902.

the same curve as the solar circulation, is directed upon the earth in polar curves as if coming from a distant spherical magnet, and not along the radial lines of electromagnetic radiation. It is not easy to account for these disturbances by flights of ions from the sun along the lines of the electromagnetic mechanical pressures. The further the discussion of the cosmical observations is pressed, the more positive becomes the evidence that the sun sustains a strong magnetic field, which responds to a variable magnetization within its nucleus. Radiation from the solar surface has another source of energy, namely, the atomic and molecular vibrations of the constituents of the outer envelope, as the photosphere, and hence much may go on at the surface which is not immediately representative of the state of the nucleus. Thus, the outpouring of heat, light and the ions streaming along the radii of electromagnetic pressure, together with the curved rays seen in the corona, consisting of positive and negative charges of electricity moving about a magnetic field, may take place at a given time in one way, while the nucleus is operating temporarily in another manner. Thus there may be divergences instead of synchronisms between the individual outbursts of spots and prominences on the solar surface as compared with the terrestrial magnetic storms and auroral displays which proceed from the nucleus, without in the least invalidating the claim that in general substantial synchronism exists. When sufficiently long intervals are taken, as a year, or possibly a few months, the conditions of the earth's atmosphere are affected by and vary with the changes in the solar processes. There has been much confusion in scientific writings arising from the failure to distinguish between physical actions at the surface and the interior of the sun, and many unsound criticisms have been published in consequence of it. The problem is complex, but with the growth of reliable data it is becoming yearly more promising of a satisfactory solution, and it is always interesting.

The Circulation of the Earth's Atmosphere. (Fig. 3.)—The meteorological theories of the motions of the atmosphere of the earth are now in a transition state; the old are passing away, and new ones are being constructed. Ferrel's theories of the structure of cyclones and anticyclones, as well as of the general cyclone of the hemisphere, have crumbled under the strain of modern observations. The 'Cloud Report' of the Weather Bureau, 1898, discarded both the Ferrel and the Oberbeck local and general vortices, and indicated a new path of research. The International Meteorological Committee has at last reached the same conclusion. (See 'Reports' for 1902 and 1903.) The problem at present is one of rebuilding in conformity with the facts. The general equations of motions were very briefly discussed by H. von Helmholtz, who introduced into them potential temperatures, in place of the density, and the corresponding constant angular momenta. From these equations arise three distinct cases, one of which was considered somewhat fully by him. The second case has been applied by Emden to solar circulation as above indicated, and the third case has not yet been sufficiently recognized by any one. Case I. shows that there are discontinuous surfaces of separation between layers having different temperatures and velocities, and that in the earth's atmosphere these should extend from about latitude 35° towards the poles, rising higher above the surface with progress poleward. Case III. gives surfaces sloping towards the earth from the equator up to about latitude 35°. This system differs entirely from Ferrel's, which adopted the canal theory of circulation with poleward currents at high elevations. These do not in fact exist, but there is evidence that the surfaces here specified are in conformity with the observed circulations as modified by mixtures. The local cyclones of the temperate zones are built up of counter currents of different temperatures derived from these general conditions, which in low levels near the surface of the ground underflow the eastward drift of the upper strata. The configuration of the isobars of the local cyclones observed on the sea level extends upward two or three miles with diminishing intensity, till absorbed in the system of normal isobars pertaining to the season of the

year. These two sets of isobars have now been separated from each other, and the proof of this statement is positive. (See 'Barometry Report, 1901; *Monthly Weather Review*, January, 1903; and another forthcoming report.) The prevailing stream lines, velocities and temperatures in high levels have been determined for the United States (see 'Cloud Report,' 1898), and are being worked up for the West Indies (report in preparation). The potential temperatures can be computed for both regions from the data in hand, and they are such that the heat of the upper strata of the temperate zones, where there is eastward flow increasing with the height, is above the quantity called for by the adiabatic law. In the tropics, with westward velocities diminishing upward, the heat of the upper strata is probably below the adiabatic quantity, though this remains to be determined. We have had since December, 1902, daily isobars for the United States on the three planes, the sea level, the 3,500-foot, and the 10,000-foot planes, and the result of the intercomparison of their varying configurations throughout the year is in conformity with this analysis. They possess much advantage in practically forecasting the areas of precipitation, the direction of storm tracks, and the rapidity of the propagation of the cyclonic areas over the United States. FRANK H. BIGELOW.

WEATHER BUREAU,
November 30, 1903.

HORTICULTURAL VARIETIES OF COMMON CROPS.

THE improvement of farm crops by breeding and selection has received a marked impetus in recent years, due partly to the success secured by a few pioneer workers in this field, and partly to recent discoveries in the laws of heredity. The present note is written for the purpose of calling attention to a method of improvement that has been applied to ordinary field crops only to a very limited extent, but which offers promise of immediate and marked results. It can be best illustrated by giving actual cases. Dr. A. D. Hopkins, at present connected with the Bureau of Entomology of this department, formerly of the West Virginia Experiment Station, for many years grew timothy for seed. For this purpose the crop is ordinarily sown thinly, so that, during the first harvest year, the plants are sufficiently distinct to permit of the observation of individual plants. Many years' close observation showed that the crop consists of a large number of constantly recurring forms quite easily distinguished. A number of plants, each representing one of these forms, were taken up and separated into as many parts as the nature of the case permitted; in this way each plant became the parent, by divisions, of a large number of plants, all set side by side in a plat. When seed was harvested from these plats it was found that the plants produced from these seeds reproduced faithfully the characters of the original selection. Each original selection, therefore, became the parent of a variety. Several of these varieties are now growing in the grass garden of the Department of Agriculture, where they have been the object of careful observation during the past season. They differ markedly in character of growth, earliness, size, etc. Some of them are evidently far superior to the ordinary timothy as grown by farmers (which is a mixture of superior and inferior varieties), some for seed production, others as hay plants, and others as pasture plants.

In a manner exactly similar, Mr. A. B. Leckenby, director of the Eastern Oregon Experiment Station, has isolated ten varieties of brome grass (*Bromus inermis* Leyss.), as distinct, for instance, as the ordinary varieties of wheat. He has also isolated a larger number of varieties of *Poa pratensis*, differing to a remarkable degree in character of growth, and consequently in agricultural value.

This method of securing new and stable varieties is probably applicable to all unimproved crops that are ordinarily close-fertilized. In the case of cross-fertilized species, a different procedure would be necessary; but if Mendel's law holds in these cases, similar results can be secured even in cross-fertilized species by artificially close-fertilizing the plants. In this case, the plants would immediately split up into a number of stable forms that could be segregated as varieties by isolating them from other forms.

The origin of these varieties which are

found in stable form in close-fertilized species (and which exist potentially in cross-fertilized species) is a matter of great interest, both theoretically and practically. The adherents of the mutation theory will see in them a confirmation of their views. The rest of us are compelled to admit that, thus far, their origin is obscure.

In the light of the facts cited, the question whether a given crop is cross- or close-fertilized becomes a matter of prime importance, as different methods of procedure are required in the two cases. Dr. Hopkins states that clover plants selected in a manner analogous to that described for timothy did not reproduce true to seed, but that the plants grown from the seed of a single plant represented all the forms observable in the original field of clover. This is what Mendel's law leads us to expect, if clover is cross-fertilized, a matter which has recently been called in question. It is easily seen that we have here a list of important problems for plant physiologists, in determining definitely what crops do and what do not cross-fertilize. There is likewise a broad and promising field of work in securing in a stable form superior strains of all ordinary crops to which these methods have not already been applied. The amount of improvement possible represents the difference between the mixture of all strains and the best components of the mixture.

W. J. SPILLMAN.
U. S. DEPARTMENT OF AGRICULTURE.

RECENT ZOOPALEONTOLOGY.

FIELD EXPEDITIONS DURING THE PAST SEASON.

THE Kansas chalk was visited by three parties during the summer. The first, under Professor S. W. Williston, representing the University of Chicago, was extremely successful, especially in procuring remains of mosasaurs, pterosaurs and toothed birds; the collection will be arranged principally as a study collection in the university. The second party represented the Carnegie Institution of Pittsburg, and is reported to have been very successful also. The third party was that of Mr. Charles H. Sternberg in the same field. He writes that he collected over sixty specimens of Cretaceous fossils, including especially well-preserved specimens of the turtles. *Protostega gigas* is represented by three skulls and a complete skeleton. The skeleton lay on its dorsal surface with the fore limbs stretched out at right angles to the median line of the carapace, measuring six feet between the ungual phalanges; the hind limbs were parallel with the neural arch, and stretched out behind. Mr. Sternberg also secured a number of mosasaur skulls, with portions of the skeleton of *Platecarpus* (one individual included sixty-six continuous vertebræ behind the skull); also skulls of each of the three genera of mosasaurs, the skeleton of *Portheus,* and skulls and skeletons of a number of other genera of fishes. It appears that erosion of the chalk is quite rapid, and there are practically fresh exposures in many parts of this famous region.

Professor Loomis, of Amherst College, who has been for some years with the American Museum of Natural History expeditions, during the past season conducted a party from Amherst into South Dakota. A collection including the remains of some 500 animals was made, chiefly in the White River beds, the best specimens being the skeleton of a titanothere and of an oreodon.

Princeton University sent an expedition under Dr. Marcus Farr into the Laramie and Judith River Beds of Montana. It is reported as having been very successful.

The American Museum of Natural History sent four parties into the field. The first, the third Whitney Expedition for fossil horses, worked in western Nebraska and South Dakota, and added considerably to the collection of fossil horses already in the museum. The choicest specimen found by this party was the skeleton of *Camelus occidentalis.* The second party worked in the Bridger Beds of western Wyoming under Mr. Walter Granger, and was successful in securing a representative collection of the small fauna of that region. The third party, under Mr. Peter Kaison, continued the excavation of the Bone Cabin Quarry in the Como region, the chief discoveries being a fore limb of *Morosaurus,* a skull of *Diplodocus,* portions of another skeleton of *Stegosaurus* and a very large

collection of the limb bones of *Camarasaurus* from the Reed Quarry. The fourth expedition went into South Dakota and northern Wyoming, under Mr. Barnum Brown, and resulted particularly in the discovery of abundant mosasaur and plesiosaur material.

The explorations of the Carnegie Museum have been described by Mr. Hatcher in a recent number of SCIENCE.

Mention should also be made of the continuation of the explorations in the Triassic under Professor Merriam, of the University of California, as well as of the cave fauna in Shasta County, a description of which has already appeared in SCIENCE. H. F. O.

BOTANICAL NOTES.

THE MISSOURI BOTANICAL GARDEN.

THE appearance of the Fourteenth Annual Report of the Missouri Botanical Garden covering the year ending December 31, 1902, enables us to note the rapid growth of this institution. The report shows that the income from all sources for the year was $127,142.50 and that considerably more than one half of this amount was expended on the garden, including library, herbarium, salaries, etc. The total number of species of plants now in cultivation in the garden is 11,551, which is more than double the number grown in 1895. The herbarium now includes 427,797 specimens. During the year there were added no less than 62,844 sheets of specimens. The botanical library was increased by more than 2,000 books and pamphlets, bringing the total number up to about 42,000. Other interesting statistics are given, showing that the garden has been an active agent in the promotion of botanical knowledge.

The bulk of the volume is taken up with a paper by Alfred Rehder under the title of 'Synopsis of the Genus *Lonicera*,' covering 206 pages, and including twenty full-page plates. The lapse of seventy years since the last general revision of the genus in the fourth volume of DeCandolle's 'Prodromus' makes such a paper as this especially necessary. This is shown by the fact that of the 154 species recognized in this monograph, but 42 occur in the 'Prodromus.' The conservative treatment accorded to the genus is indicated by the small number of new species (eleven, only) which the author has described. Such moderation, after the 'lying fallow' of this particular botanical field for so long a time, should put to shame our 'species makers.' In this the Missouri Botanical Garden has rendered a distinct service to botanical science.

AN ELEMENTARY JOURNAL OF MYCOLOGY.

ABOUT a year ago Professor Kellerman, of Columbus, Ohio, began publishing a leaflet for the benefit of those who wish to learn something about the fungi. He called it the *Ohio Mycological Bulletin* and filled it with excellent photoengravings of the larger fungi. With each picture was given a simple description adapted to the understanding of 'children in years and children in knowledge.' It has been so successful that practically all of the earlier numbers have been exhausted. The first volume, which includes twelve numbers aggregating forty-eight pages, closes with a good index. With the last number a title page for the volume is supplied. The new volume is to start with the new year, and it is announced that 'the frequency of issue during the year will depend on the financial receipts.' The hope is expressed that two numbers a month may be issued during the spring and fall. For teachers in the public schools who wish to learn to know the commoner large fungi nothing better than this is published anywhere.

SOME RECENT PAPERS ON SYSTEMATIC BOTANY.

WILLIAM R. MAXON in the 'Contributions from the United States National Herbarium' (Vol. VIII., part 3) publishes 'A Study of Certain Mexican and Guatemalan Species of *Polypodium*,' in which he notices eight species, five of which are new to science. Two good plates illustrate the paper.

In the September number of the *Bulletin of the Torrey Botanical Club* Dr. G. N. Best revises the mosses of the genus *Leskea*, so far as the North American species are concerned. Ten species are recognized, two of which are new. He finds two new varieties also. The paper is accompanied with two plates showing structural details.

In the October number of the *Journal of the Linnean Society* the 'Enumeration of all the Plants known from China Proper, Formosa, Hainan, Corea, the Luchu Archipelago and the Island of Hongkong,' by Francis B. Forbes and William B. Hemsley, is carried forward nearly through the Cyperaceæ. As the sequence is that of Bentham and Hooker, it is likely that a few more numbers will see the end of this great work.

In No. 247 of the *Journal of the Linnean Society* (dated October, also) W. and G. S. West publish an interesting paper on the 'Scottish Freshwater Plankton,' which shows that the Scottish phytoplankton 'is unique in the abundance of its desmids.'

CHEMISTRY OF PLANT AND ANIMAL LIFE.

PROFESSOR SNYDER, of the University of Minnesota, has compiled a handy little volume under the title of 'The Chemistry of Plant and Animal Life,' which merits a notice here, since it is an attempt to place within reach of the beginner many of the chemical facts which otherwise are inaccessible to him. It is an elementary treatise and was originally prepared for the students in the school of agriculture of the university. This made it necessary that the treatment should be quite simple, and as nearly non-technical as possible. It is not, therefore, a 'contribution' to science, but it is a contribution to the pedagogics of science. The author has found how to present the subject for the class of students under consideration; a class characterized by great earnestness and a desire to learn all that can be reached, but whose scholastic preparation is somewhat defective. Difficult as is the problem, Professor Snyder has successfully solved it. He first gives about twenty chapters to a simple statement (with experiments) of general chemistry, and follows these with such topics as 'the water-content of plants,' 'the non-nitrogenous organic compounds of plants,' 'the nitrogenous organic compounds of plants,' 'chemistry of plant growth,' 'composition of fodders,' 'composition of wheat,' etc. The book, while a simple one, and no doubt here and there open to the criticism of some confusion of details, is without question one which will be of great service to beginning students, especially in the schools of agriculture. A new edition is under way, and is to appear soon. It should find place in many schools.

CHARLES E. BESSEY.

THE UNIVERSITY OF NEBRASKA.

THE CARNEGIE INSTITUTION.

THE trustees of the Carnegie Institution have approved the recommendation of the executive committee that $10,000 be granted for twenty tables at the Marine Biological Laboratory at Woods Hole, Mass., for 1904. Applications received prior to February 1, 1904, will be considered, and twenty persons assigned to the tables at the laboratory, for the season of 1904.

The trustees have also approved of an appropriation for two tables at the Naples Marine Biological Station, for which applications will be received and considered up to February 1, 1904.

It is desirable that all applications for research assistantships shall be in the hands of the committee by February 1.

The regulations in regard to the research assistantships are as follows:

It is the purpose of the Carnegie Institution of Washington, among other plans, to encourage exceptional talent by appointing a certain number of research assistants.

These positions will not be those commonly known as fellowships or scholarships; nor is the object of this provision to contribute to the payment of mechanical helpers or of assistants in the work of instruction. It is rather to discover and develop, under competent scrutiny and under favorable conditions, such persons as have unusual ability. It is not intended to provide means by which a student may complete his courses of study, nor to give assistance in the preparation of dissertations for academic degrees. Work of a more advanced and special character is expected of all who receive appointment.

The annual emolument will vary according to circumstances. As a rule, it will not exceed $1,000 per annum. No limitations are prescribed as to age, sex, nationality, graduation or residence. Appointments will, at first, be made for one year, but may be continued.

It is desirable that a person thus appointed should work under the supervision of an investigator who is known to the authorities of the Carnegie Institution to be engaged in an important field of scientific research, and in a place where there is easy access to libraries and apparatus—but there may be exceptions to this.

Applications for appointments may be presented by the head of, or by a professor in, an institution of learning, or by the candidate. They should be accompanied by a statement of the qualifications of the candidate, of the research work he has done, and of that which he desires to follow, and of the time for which an allowance is desired. If he has already printed or written anything of interest, a copy of this should be enclosed with the application.

Communications upon this subject should be distinctly marked on the outside envelope, and on the inside, 'Research Assistant,' and should be addressed to the Carnegie Institution of Washington, Bond Building, Washington, D. C.

SCIENTIFIC NOTES AND NEWS.

As all our readers know, the American Association for the Advancement of Science, the American Society of Naturalists and about twenty affiliated societies are meeting this week at St. Louis. Several of the most important national societies devoted to the biological sciences, or their eastern branches, are meeting in Philadelphia. The American Philosophical Association is meeting at Princeton, and there are more or less local meetings in other cities. At the time of going to press information in regard to these meetings has not reached us; but we shall as usual publish full reports in the issue of next week and in subsequent issues.

M. Emile Bertin has been elected a member of the Paris Academy of Sciences in the section for geography.

M. H. Gréhant, professor of physiology in the Paris Museum of Natural History, has been elected a correspondent of the Philadelphia Academy of Natural Science.

Professor Ludwig Boltzmann, of Leipzig, has been elected an honorary member of the Academy of Sciences at Moscow.

Professor Otto Bütschli, professor of zoology and paleontology of the University of Heidelberg, has been appointed an honorary member of the Universities of St. Petersburg and Moscow.

The honorary doctorate of the University of Marburg has been conferred on Dr. Thedor Tschernyshew, of St. Petersburg, director of the Russian Geological Committee.

The University of Munich has conferred an honorary doctorate of philosophy on Mr. L. Cockayne, of Christ Church, New Zealand.

Mr. A. J. Eveland, a graduate student in geology and mineralogy of the Johns Hopkins University, has been appointed geologist to the Mining Bureau established by the United States Government in the Philippine Islands.

Mr. John Shafer, formerly custodian of botany at the Carnegie Museum of Pittsburg, has been appointed custodian of the Museum of the New York Botanical Gardens.

Professor Paul Ehrlich, director of the Royal Institute for Experimental Therapeutics at Frankfurt, a/M, will deliver the first course of Herter lectures at the Johns Hopkins University Medical School. Professor Ehrlich's lectures will be in German, and will probably present the results of his researches on immunity.

Dr. G. Sims Woodhead, professor of pathology at Cambridge University and member of the Royal Commission on Tuberculosis, gave the third Henry Phipps Institute lecture on December 29 at Philadelphia, his subject being 'Paths of Infection in Tuberculosis.'

The Bradshaw lecture was delivered before the Royal College of Surgeons on December 9 by Mr. Henry Morris, the subject being 'Cancer and its Origin.'

A special meeting of the Scottish Geographical Society was held at Edinburgh on December 17, under the presidency of Professor James Geikie. An address was delivered by Sir Thomas H. Holdich on 'The Patagonian Andes.'

A CABLEGRAM to the daily papers states that Dr. Alexander Graham Bell arrived at Genoa on December 27. He will convey to the Smithsonian Institution at Washington, D. C., the remains of James Smithson, founder of the institution, who died in Genoa in 1829.

WE learn from the *Botanical Gazette* that the large herbarium of the late Professor C. Haussknecht will be maintained by his family under the auspices of the Thuringian Botanical Society.

IT is announced that Dr. Oscar Guttmann has presented to the London Chemical Society a photograph of the portrait of Roger Bacon in possession of Lord Sackville at Knole House, Sevenoaks.

DR. FRIEDRICH GOLL, professor of pharmacology at Zurich, has died at the age of seventy-three years.

WE regret to record the death of M. Proust, professor of hygiene of the University of Paris and inspector general of the Sanitary Service; of Dr. Eugene Askenasy, honorary professor of plant physiology at the University of Heidelberg; and of Dr. Ottmar Schmidt, professor of chemistry in the Institute of Technology at Stuttgart.

DR. P. CHALMERS MITCHELL, secretary of the London Zoological Society, writes to the London *Times:* The recent death of the Polar bear, a popular favorite at the Zoological Gardens since 1895, has caused some interest and has been the occasion of many published comments based on inaccurate information. Perhaps you will allow me space to state the facts. The bear was in good health and spirits and fed well until the afternoon of Sunday, November 1, when, soon after taking food, it fell backwards and died almost instantaneously. The *post-mortem* changes were unusually rapid, and next day an examination was made in the presence of Mr. Beddard, the society's prosector, and myself, and a preliminary diagnosis was arrived at. Subsequently Dr. Salaman, pathologist to the London Hospital, a fellow of the society, who has very kindly placed his services at the disposal of the society until the return from abroad of the special pathologist recently appointed by the council, made a careful examination of the material that we had reserved, and established the correctness of the preliminary diagnosis that the cause of death was an aortic aneurism. The case was of great scientific interest, and Dr. Salaman will communicate to a future scientific meeting of the society a detailed account of it. I may say now, however, that, except for the local lesion, the organs and tissues were healthy, and it is extremely improbable that the creature suffered. It would have been impossible to make the diagnosis during life, or, had we known of the existence of the disease, to have taken any steps for its treatment. I may add that, while in the past very considerable additions to anatomical knowledge have been made at the prosectorium attached to the gardens, the council of the society, by increasing the accommodation for pathological work and by appointing a special pathologist, hope that additions to knowledge of the treatment of animals will be made.

BARON EDMUND DE ROTHSCHILD has placed in the hands of M. Albert Gaudry, president of the Paris Academy of Sciences the sum of 10,000 francs to enable him to secure for the Paris Museum of Natural History the more valuable specimens in the Filhol paleontological collection.

ACCORDING to Reuter's Agency, Mr. Bruce, the leader of the Scottish Antarctic Expedition which was sent out last year on board the *Scotia,* has arrived at Montevideo from the Falkland Islands. He reports that all is well in the *Scotia,* which is on the way to Buenos Ayres. Six men have been left behind in charge of a meteorological station. The news of the safe return of the Scottish Antarctic Expedition has come some two or three months earlier than was expected. It was not originally Mr. Bruce's intention to winter in the Antarctic, but it was understood that if he did so nothing would be heard of the expedition after its departure from Port Stanley, Falkland Islands, until March of next year. The meteorological station referred to by the explorer at which six of his men have been left appears to be the station set up by Mr. Bruce at Cape Pembroke, Falkland Islands,

before the *Scotia* left for the southern seas in January last.

THE National Geographic Society has recently moved into its new home, the Gardiner Greene Hubbard Memorial Hall. As the building is not entirely completed, the formal opening of the hall will be deferred for the present. The society offers three courses of meetings during the season of 1903–1904—a regular or scientific series of ten meetings; a popular series of ten illustrated lectures, and an afternoon or lenten series of five popular lectures.

A SPECIAL Roentgen Congress and Exhibition is to be held at Berlin during the spring to celebrate the tenth anniversary of the discovery of the X-rays. Professor Roentgen is expected to be present at the congress. Further information can be obtained from Dr. Immelmann, Lützowstr. 72, Berlin, W., Germany.

THE annual dinner of the Institute of Chemistry of Great Britain took place on December 14. Speeches were made by the president, Dr. Davis Howard, Sir William Huggins and Sir William Ramsay.

THE Canadian papers state that at a meeting of the board of directors of the Canadian Forestry Association, held at the office of Mr. E. Stewart, Dominion Superintendent of Forestry, the treasurer reported the receipt of a grant of $300 from the Government of Ontario to assist in the work of the association, and that the governments of Quebec and British Columbia had also promised assistance. The membership has reached the number of 420, and, with the improved financial position in which the association finds itself, it is proposed to extend the sphere of its activities. The establishment of a journal devoted specially to forestry interests was discussed, and it was decided to report favorably to the annual meeting. The publication will, if started, be managed by the association, and will probably be at first a quarterly, with the expectation of being finally issued as a monthly. It is hoped in this way to call public attention more distinctively to the work of the association, and to the importance of proper forest management.

UNIVERSITY AND EDUCATIONAL NEWS.

AT the forty-ninth quarterly convocation of the University of Chicago President Harper announced that Mr. John D. Rockefeller had given to the university $1,500,000 in real estate and $350,000 in cash. A donor, whose name was not made public, has given $1,096,466 for a special purpose not yet designated.

IT is stated that Mrs. Phoebe Hearst will provide a building for the Department of Botany of the University of California.

A GIFT of $1,000 from Edward Mallinckrodt, of St. Louis, has enabled the department of chemistry of Harvard University to refurnish the library of Boylston Hall and to buy several hundred new books. E. Mallinckrodt, Jr., has added to this a sum to be paid annually for the next five years to defray the running expenses of the library. The collection of books has also been enlarged by several gifts from Dr. Wolcott Gibbs.

WE learn from the London *Times* that the late Mr. Charles Seale-Hayne, M.P., has under his will provided for the establishment of a College of Science, Art and Agriculture in the neighborhood of Newton Abbot, open to students of the county of Devon. Details will be left to the executors. It is thought that about £150,000 will be handed over for the college.

DR. HORACE CLARK RICHARDS, instructor in physics in the University of Pennsylvania, has been promoted to an assistant professorship of physics.

MR. HENRY BALFOUR, M.A., of Trinity College, Oxford, has been elected to fellowship at Exeter College. Mr. Balfour has been for some years curator of the Pitt-Rivers Museum. He is also president of the Anthropological Institute, and president-elect of the Anthropological Section of the British Association for 1904.

DR. HERMANN GRASSMANN, docent at Halle, has been promoted to an assistant professorship of mathematics.